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of Vivisection  
Dr. Berdoe

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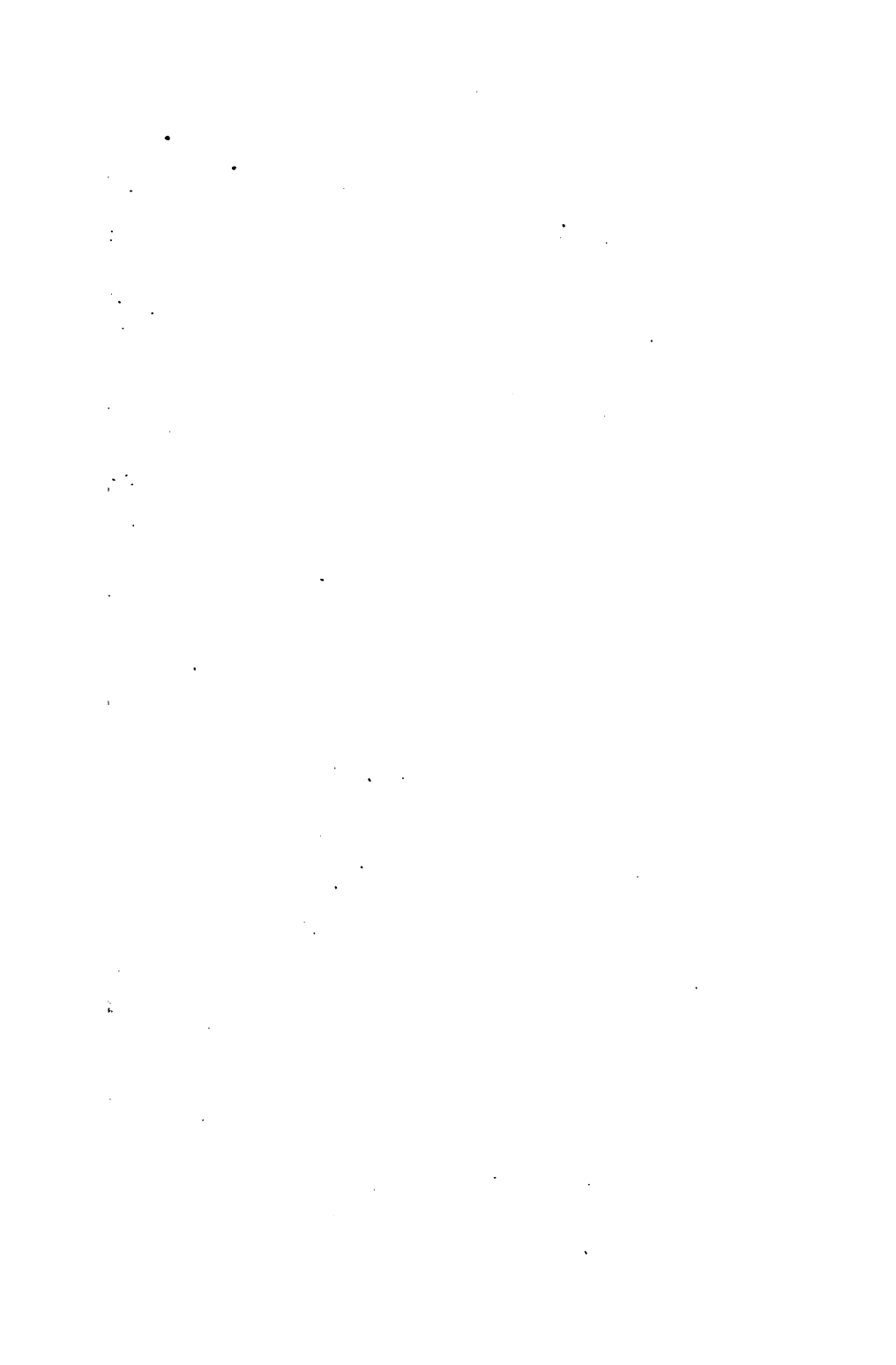


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SWAN SONNENSCHN & CO., Paternoster Square.

# A CATECHISM OF VIVISECTION.

THE WHOLE CONTROVERSY ARGUED  
IN ALL ITS DETAILS

BY

EDWARD BERDOE

M.R.C.S. (ENG.), L.R.C.P. (ED.), ETC., ETC.

*Author of "The Origin and Growth of the Healing Art,"  
"The Browning Cyclopædia," etc., etc.*



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## PREFACE

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THE Vivisection Controversy embraces such a number of questions in so many branches of knowledge, that there is probably no man living who can truthfully say that he knows the subject completely in all its ramifications. To do this he must first have a good working knowledge of the professions of medicine and surgery, and be "up-to-date" in these. Then he must have a minute acquaintance with medical history from the earliest times, and in all the countries of the world; he must have some knowledge of the history of religions, for the priest and the medical man are very near neighbours and very old acquaintances. He must be conversant with the sciences of biology, physiology, physiological chemistry, pharmacology, toxicology, and the history of civilisation and the development of the arts ancillary to medicine. Above all, he must have a logical, open, and unbiassed mind, and should possess a humane and sympathetic spirit in dealing with the ethical questions involved in the subject. Who is sufficient for these things? The present writer makes no pretensions to such knowledge. All that he claims to have done in the following pages is to have surveyed the vast field as a mining prospector, searching for valuable material, with the aid of an earnest desire to tell the truth on a much disputed question.

The apologists of Vivisection have written many voluminous treatises on the matter, which might be described as Browning speaks of certain Rabbinical writings, "A Collection of Many Fables," and it is the purpose of this work to trace them to their origin, to dispel their many mythical stories, and by applying the methods of "the Higher Criticism" to the legends and childish notions about so-called "Scientific Medicine," render a service at once to truth and morality.

EDWARD BERDOE.

LONDON,  
*June 22nd, 1903.*



# A CATECHISM OF VIVISECTION.

## THE WHOLE CONTROVERSY ARGUED IN ALL ITS DETAILS.

BY EDWARD BERDOE, M.R.C.S., L.R.C.P.ED., ETC.

### CHAPTER I.

#### ON THE REAL OBJECT OF VIVISECTION.

Q. *What is Vivisection ?*

A. Vivisection is the cutting into a living animal for scientific investigation. Besides cutting operations living animals are subjected by experimenters to inoculation, poisoning, baking, burning, starving, suffocating, and other cruel methods of research, and all such methods are included in common parlance under the term Vivisection. The Royal Commission on Vivisection described it as "The practice of subjecting live animals to experiments for scientific purposes."

Q. *Is the subjecting of animals to poisons correctly termed Vivisection ?*

A. No ; strictly speaking it is not, but in its larger sense Vivisection includes it.\*

Q. *You say that the object of Vivisection is the advancement of Science ; do you mean by that Medical Science ?*

A. It is claimed that such is the ultimate object, but the claim cannot be substantiated.

Q. *But surely the Vivisector has some clearly defined object which he attains more or less ?*

A. He has, and I prefer to call it in the words of the Royal Commission, experimenting "for scientific purposes."

---

\* *Minutes of Evidence taken before the Royal Commission on Vivisection* (Questions 3—6).

Q. *You distinguish between Science and the Arts of Medicine and Surgery?*

A. I do. Neither medicine nor surgery is, or ever can be, a Science, although both depend upon Science for their improvement. They are arts.

Q. *But there are certain sciences ancillary to the healing art, such as Physiology, Pathology, and Psychology; surely these can be advanced by experiment and thus indirectly advance the arts of medicine and surgery?*

A. I do not admit that the torture of animals is necessary for the progress of any of these sciences, so far as they can aid the physician or the surgeon.

Q. *Then why do physiologists and pathologists perform these experiments?*

A. They perform them for the acquisition of facts, irrespective of their utility to doctors. The physiologist works in his laboratory to gain knowledge; if it happen that he thus becomes possessed of certain information useful to the physician he has no objection to his using it. He is like a carpenter shaping a piece of wood; his object is not to make shavings and sawdust, but to make tables and cupboards. The shavings and sawdust are bye-product useful in their way, perhaps, but not for his purpose.

Q. *What authority have you for this?*

A. The highest, for example, Dr. Ludimar Hermann, Professor of Physiology and Medical Physics, Zurich University, said,\* "The advancement of our knowledge, and its utility to medicine, is the true and straightforward object of all vivisection. No true investigator in his researches thinks of the practical utilization. Science can afford to despise this justification with which vivisection has been defended in England."

Dr. Charles Richet, the French physiologist, said,† "I do not believe that a single experimenter says to himself, 'I give curare to a rabbit, or cut the spinal marrow of a dog, or poison a frog: 'Here is an experiment which will relieve or will cure the disease of some men.' No, in his mind he does not think of that! He says to himself, 'I shall clear up an obscure point, I will seek out a new fact.' / The scientific curiosity, which alone animates him, is expl

\* *Die Vivisectionsfrage für das grössere Publicum beleuchtet* (Leip

† *Revue des deux Mondes* (February 15th, 1883).

the high idea he has formed of science. This is why we pass our days in fetid laboratories surrounded by groaning creatures, in the midst of blood and suffering, bent over palpitating entrails."

Q. *Do English physiologists admit this ?*

A. The most eminent apologists of Vivisection in this country have said as much. Sir H. W. Acland, M.D., F.R.S., late Regius Professor of Medicine in the University of Oxford, told the Royal Commission on Vivisection\* that "modern civilization seems to be set upon acquiring, almost universally, what is called biological knowledge; and one of the consequences of that is, that whereas medical men are constantly engaged in the study of anatomy and physiology for a humane purpose—that is, for the purpose of doing immediate good to mankind—there are a number of persons now who are engaged in the pursuit of these subjects for the purpose of acquiring abstract knowledge. That is quite a different thing. But now it has become a profession to discover; and I have often met persons who think that a man who is engaged in original research for the sake of adding to knowledge is therefore a far superior being to a practising physician who is simply trying to do good with his knowledge; that he is a superior being, because he is devoted to pure research." This opinion finds its echo in the press. Thus, in a leading article in the *Times*, Dec. 27th, 1884, the writer says:—"We should hold ourselves that, subject to the plain dictates of humanity, the pursuit of knowledge itself was a sufficient justification for the infliction of pain on animals by persons duly authorised and properly qualified to conduct serious scientific inquiries."

Q. *You think this desire to gain abstract knowledge leads men to vivisect in a heartless manner ?*

A. Certainly. In the words of Sir Henry Acland, "So many persons have got to deal with these wonderful and beautiful organisms just as they deal with physical bodies that have no feeling and no consciousness." †

Q. *The Vivisector in pursuit of mere knowledge looks upon his victim as the chemist looks upon his test tubes and reagents, and treats them accordingly !*

A. Precisely. Claude Bernard, the great French physiologist, said ‡ "A physiologist is no ordinary man. He is a learned man,

\* *Evidence, Royal Commission, Q. 944.*

† *Ibid.*

‡ *Introd. à l'Etude de la Médecine Experimentale* (Paris, 1855), p. 180.

a man possessed and absorbed by a scientific idea. He does not hear the animals' cries of pain. He is blind to the blood that flows. He sees nothing but his idea, and organisms which conceal from him the secrets he is resolved to discover." Dr. Cyon, Professor of Physiology at St. Petersburg, said much the same: "The true vivisector must approach a difficult vivisection with joyful excitement. He who shrinks from cutting into a living animal, he who approaches a vivisection as a disagreeable necessity, may be able to repeat one or two vivisections, but he will never be an artist in vivisection. The sensation of the physiologist when, from a gruesome wound, full of blood and mangled tissue, he draws forth some delicate nerve-thread . . . has much in common with that of the sculptor."\*

*Q. But why do men who have devoted their lives to physiological science practise vivisection if it can only gratify their taste for abstract knowledge? Men do not usually waste their energies like this.*

A. They do not waste their energies at all. Abstract science has many rich rewards to bestow on her votaries—rewards of a very tangible character. Original research in physiology gains the Fellowship of the Royal Society; the magic letters "F.R.S." appended to the name of a scientist confer on the owner a distinction that is certain to be followed by position and emoluments.

*Q. What do you mean by "biological" and "abstract knowledge" in this connection?*

A. Biology deals with the science of life in general and is equally concerned with the nervous system of a lobster and that of a man. Abstract knowledge is knowledge that has no relation to the practical side of medicine; it is knowledge for its own sake; science altogether apart from results.

*Q. But may not great medical and surgical benefits accrue from the pursuit of this disinterested kind of knowledge?*

A. They may; but in the case of experimentation upon living animals there is no evidence to prove that it has conferred such benefits.

*Q. Why then do the vivisectors proclaim so loudly and persistently that their whole and sole object is to benefit the healing art by their discoveries?*

A. This is easily answered. The public sanction would never be given to the cruelties of the torture chambers of

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\* *Methodik*, p. 15.

science unless some great boon to humanity were promised in return.

*Q. The appeal is to our selfishness ?*

A. Exactly. The doctors assure us that the experimenters are ever working to provide them with the means of curing our diseases, and that the advancement of medicine is cheaply secured by the sufferings of a few animals.

*Q. You deny that the promise is fulfilled ?*

A. I do. There has never been any discovery made in consequence of experiments upon animals of importance enough to justify the cruelties practised in a single laboratory in a single year.

*Q. You would find few medical men to support you in this contention. How do you account for that ?*

A. The great majority of the members of the medical profession know very little about the Vivisection controversy ; the subject is a highly technical one, and has so little bearing on the practical side of the busy doctors' daily work that they may well be excused for neglecting it. Naturally, however, they are unwilling to confess their ignorance, and so for the most part they range themselves with the scientific leaders of the profession who are practically all on the side of what is called research.

*Q. How do you account for this ?*

A. The present is the age of science. The art of medicine lags behind the progress of the time. At best it is but a wise empiricism. The most successful doctor can seldom tell why he cures or fails to cure disease ; it is still the fact that " he pours drugs of which he knows little into bodies of which he knows less." To elevate medicine from the reproach of empiricism to the status of a science is therefore the aim of the heads of the medical profession, and they hope to do this by an increased knowledge of biology and physiology.

*Q. Has Science done nothing for the public health ?*

A. Certainly it has done much. Improvements in sanitary Science, hygiene, and the management of infancy have largely decreased mortality at all ages, especially in towns. Abundant supplies of pure water, good drainage, increased air-space in factories and in dwelling rooms, purer and more abundant food for the masses, are amongst the principal factors in reducing the death-roll, but Vivisection has had nothing to do with these things.



## CHAPTER II.

## VIVISECTION AN UNSCIENTIFIC METHOD.

*Q. Although it may be as you say that physiological research has hitherto done little for practical medicine, I am by no means convinced that it is useless to pursue it. Analysis must precede synthesis, and arguing from analogy the energizing scientific spirit is ultimately always fertile in practical results. Is it not so?*

*A. I deny that Vivisection is a true scientific method. The worm at the root of the matter is the impossibility of arriving at the normal from the study of life under abnormal conditions.*

*Q. Explain. I do not quite see what you mean.*

*A. There is all the difference in the world between interrogating Nature and putting her to the question. Joan of Arc in the torture chamber said, "I will tell you nothing more than I have told you; no, not even if you tear the limbs from my body. And even if in my pain I did say something otherwise, I would always say afterwards that it was the torture that spoke and not I."*

*Q. This is a point. Can you illustrate it?*

*A. The old Roman physician Celsus said, "If, in the entire and uninjured body we can often, by external observation, perceive remarkable changes, produced from fear, pain, hunger, weariness, and a thousand other affections, how much greater must be the changes induced by the dreadful incisions and cruel mangling of the dissector, in internal parts whose structure is far more delicate and which are placed in circumstances altogether unusual." (Book III.)*

*Q. I suppose he was referring to the criminals who were given over to experimenters in the physiological laboratories of the medical school of Alexandria?*

*A. No doubt. But consider, if fear, terror, and shock can blanch the cheek, paralyse the limbs, make the teeth chatter and turn the hair suddenly white, and so alter the physiological condition of one human being that in a few days or hours he is a different creature from his normal self, how much more unlike the normal man must be the tortured dog or rabbit in*

the laboratory, who in his natural state differs so widely from ourselves. The very nature of this method of research opens the door to innumerable fallacies. Every strong mental emotion, every phase of health and disease, the changes of temperature, even our attitudes and occupations affect our respiration. What then must be the abnormal condition of an animal's breathing when it is stretched upon a torture trough, its internal organs exposed by vivisection to the air, its nerves laid bare to artificial stimuli, its whole frame convulsed with agony or its senses stupefied with drugs?

*Q. I have heard that acute suffering will alter the blood and flesh of an animal so that they become poisonous, is that a fact?*

A. Undoubtedly it is. Baron Liebig, in the appendix to his *Letters on Chemistry*, says:—"A roe-buck was caught in a snare, and the abdomen and pelvis of the animal being enclosed in the cord, it must have died after a most agonizing struggle. On eating its flesh, symptoms resembling those caused by the bites of rabid animals manifested themselves—the husband recovered after three months' illness; the wife died after lingering more than two years; the daughter, manservant, and maid who had eaten but little of the tortured animal, were soon cured." Under certain circumstances milk may become poisonous. In *Taylor on Poisons* a case is mentioned in which a woman who had become violently excited over some domestic trouble suckled her child while in this agitated condition with the result that it died. Many cases are on record of jaundice, skin eruptions, and other maladies having been caused by fright and shock.

*Q. But may not the Vivisectors be doing pioneer work for medicine? Are they not exploring and sifting out new materials, collecting facts, and ascertaining laws? If I mistake not, this is just what Faraday did for electrical science. He did not give us electric railways nor illuminate our streets and houses, but the facts he accumulated and the laws he arrived at made it possible for his successors to do all this for us. May not the physiologists be the Faradays of the coming medicine?*

A. This is very plausible, but it contains a great fallacy. The laws of electricity like those of chemistry are definite and can be formulated inerrably. A chemical or electrical experiment properly carried out with the same materials and apparatus always yields the same result. The reagents produce in the test-tube in London, Berlin, or Calcutta always the identical substance when acting in the same solutions; acetate of lead acted upon by iodide of potassium is always converted into yellow iodide of lead; there is no

variation, the change infallibly takes place, but the permanent and unvarying properties of inanimate matter are quite unlike the qualities of living, sentient beings. The principle of life in the latter makes all the difference in the world. As Dr. James Macaulay has well said, \* "The results obtained by different experimenters are so various, and often so contradictory, that there is scarcely a single position laid down by them that can with confidence be adopted. We find that the most opposite results occur at different times from injury of the same organs, that injuries of different organs often produce the same results, and that the same experiments are not followed by the same results in different subjects."

*Q. If that is so, and I shall ask you to illustrate it later on, I cannot see how error can be avoided in these researches till we discover all the causes which lie behind the varying results. Reasons for them there must be; science will ultimately discover them. Am I not right?*

A. There must be reasons for many things we have little hope of finding out. The greatest biologists tell us that we have no hope of bridging the gulf between the living and the not living. Scientists tell us that the tendencies of modern physiology lead towards the conclusion "that consciousness is a function of the grey cellular substance of the brain," but this will never help us to understand how our food is transmuted into thought.

*Q. Yet we are daily accumulating vast stores of physiological facts, is there not some encouragement in that?*

A. Very little, so far as practical results are concerned; for instance, Dr. Carpenter, the physiologist, once said, that Professor Schiff, of Florence, had vivisected fourteen thousand victims and had not made one discovery big enough to be explained to anyone not a physiologist—nor were any of them of the least practical value.

*Q. Does not the experimenter aim at ascertaining the functions of organs taken by themselves, and the influence of drugs and other things on those structures separated from the rest of the animal body, rather than studying its complicated mechanism as a whole?*

A. That is so. But the doctor has to treat his patient as a whole, not the heart, the brain, or the liver by itself, but the man in whom these organs exist as part of the organism.

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\* *Vivisection* (Prize Essays), p. 62.

*Q. But how can heart or liver disease be scientifically treated, unless we know precisely in what manner these organs are influenced by the remedies we use?*

A. As long as the patient is cured and his remedies work well, it does not matter much whether he is treated empirically or scientifically.

*Q. Can you illustrate this?*

A. Dr. Lauder Brunton says in his great work on Pharmacology\*, "Perhaps the best example of the empirical use of a remedy is that of quinine in ague. We do not know with certainty what the pathological conditions are in this disease, nor how quinine acts upon them; all we know is that it has proved useful in cases of ague before, and therefore we give it again."

*Q. The aim of the experimental physiologists I presume is to substitute rational medicine for empiricism?*

A. This is what they profess. Dr. Burdon-Sanderson told the Royal Commission that "just as completely as mechanical science has come to be the guide of mechanical arts, do I believe that physiological science will eventually come to be the guide of medicine and surgery."†

*Q. Do vivisectors generally believe that?*

A. No; for example, Prof. Rutherford said to the Royal Commission,‡ "I must remind you that medicine can never be perfectly exact. We shall never be able to say that in two different individuals with livers in a state of torpidity, from different causes, that the same dose of rhubarb or of podophyllin will produce the same effect, or even, whether in any case they will produce the same marked effect as they do in dogs."

*Q. Do you not think that every doctor should be a skilled physiologist?*

A. I do not. Professor Michael Foster, of Cambridge, said, "Nothing is to my mind more dangerous than the practice of taking, so to speak, 'raw' physiology at once into clinical work; and if you look back on the history of the profession you will find that these attempts to take

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\* *A Text-Book of Pharmacology, Therapeutics, and Materia Medica*, by T. Lauder Brunton. Introduction, p. i.

† *Report of Royal Commission on Vivisection* (2047).

‡ *Ibid* (2959).

physiology straight off into practical use have proved defusive, and to my mind a great deal of discredit has been thrown upon physiology by these attempts."\*

Q. Did not Professor Huxley once say something of the same kind?

A. He did. In a discussion in *The Times*† with Mr. Herbert Spencer he said, "Mr. Spencer assumes that in the present state of physiological and medical science, the practitioner would be well advised who should treat his patients by deductions from physiological principles ('absolute physiological therapeutics' let me say) rather than by careful inductions from the observed phenomena of disease and of the effects of medicines. Well, all I can reply is, Heaven forbid that I should ever fall into that practitioner's hands; and if I thought any writings of mine could afford the smallest pretext for the amount of manslaughter of which that man would be guilty, I should be grieved indeed."

Q. Will you tell me what physiologists themselves allege to be the principal use of their experiments?

A. Dr. Lauder Brunton has stated them in very plain and precise terms. In his *Pharmacology and Therapeutics* he says: ‡ "The problems put before us are too complicated to be solved directly and we must therefore simplify them. This is done in four ways:—

"(1) By observation of the effects of drugs on animals with a simpler organism than our own, such as amœbæ or frogs;

"(2) By applying the drug to some part of an animal's body more or less completely separated from the rest, such as, for example, the muscle and nerve, or the heart of a frog separated from the body; and,

"(3) By preventing it from reaching one part of the body while it acts on the others, as by ligaturing an artery as in Bernard's or Kolliker's experiments on curare.

"(4) By producing artificial changes in the relations of the various parts of the body of higher animals, either before or after administration of a drug, as, for example, by dividing

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\* From an Address before the Medical Society of University College, London, on the question, "Why should Medical Students Study Physiology?" *The Lancet*, October 19th, 1889.

† See *The Times* (November 15th, 1889, and November 18th, 1889).

‡ *A Text Book of Pharmacology and Therapeutics*, 1885, p. 37.

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the vagi, in order to ascertain how far the change produced in the beats of the heart by a drug is due to its action upon it by the nerves."

*Q. Would not a physician educated on such lines as these make many grave mistakes in treating his patients ?*

A. Naturally, and I shall have on another occasion to explain several of them to you.

## CHAPTER III.

### THE BLIND GUIDES OF THE LABORATORY.

*Q. You promised to give me some examples of the errors into which experimenters have led medical men.*

A. That will be a congenial task to me. Man is an erect animal, he does not go upon all fours, like the animals used for experiment, yet, strange to say, physiologists have, for the most part, ignored this important fact in arguing from the lower animals to man.

*Q. Does the position of the trunk make any important difference in the physiology?*

A. Certainly it does. Dr. A. Morison recently pointed out in connection with the subject of the Blood Pressure that experiments had not been carried out under the conditions that obtain in Nature. He said, \* "The arguments from quadrupeds to man as to the power of the heart and pressure in the blood vessels are fallacious, for the physical reasons which bring about a difference in the circulatory apparatus of animals habitually and respectively horizontal and erect."

*Q. Was not some very serious error in the treatment of hernia introduced into surgical practice by some experiments on dogs, performed by Mr. Travers?*

A. Yes, and it was fully explained before the Royal Commission † by Mr. George Macilwain, F.R.C.S. who said, "Mr. Travers made experiments on dogs. . . He divided the intestines and sewed up the wounds . . . and did a great many things of that kind, and showed that animals have certainly very great powers of repair under such circumstances. But now the inductive philosophy comes in. He left out some of the most important parts of the subject as regarded the human subject, because he never purged these dogs. If he had wanted to carry the analogy close for strangulated hernia he should have placed the dog . . . as nearly as possible under the same circumstances as the human subject. . . . In the same book in which Mr. Travers published the experiments

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\* *British Medical Journal* (March 14th, 1896), p. 650.

† *Blue Book*, QQ. 1,848—1,852.

he also published the treatment of strangulated hernia; he gave his directions for the treatment which is exactly that which destroys the patient."

Q. *Mr. Travers, after his experiments, made the terribly fatal error of giving purgatives in cases of strangulated hernia; is that so?*

A. Yes. And Mr. Stanley, of St. Bartholomew's Hospital, in lecturing to his pupils some twenty years afterwards, or more than that, says as follows\*: "That at one time purgatives were employed in these cases, whereas it is now perfectly understood that they ought not to be so employed, and he had himself had bushels from cases of strangulated hernia—cases where the fatal peritonitis was traceable to the purge."

Q. *Did not Mr. Macilwain express himself very strongly about this?*

A. He said, "I know of no error in the whole practice of surgery . . . which has produced an evil equal to that which I myself have practically corrected, and that is the employment of purgatives after the operation in strangulated hernia."

Q. *Has not Mr. Lawson Tait—who was, I believe, one of our greatest authorities on the surgery of the abdomen—pronounced a very decided opinion on a somewhat analogous subject?*

A. He has. In a letter addressed to myself, dated October 9th, 1889, he said† "You may take it from me that instead of vivisection having in any way advanced abdominal surgery, it has on the contrary had a uniform tendency to retard it."

Q. *Has he given any examples of that?*

A. In a letter published in the *Birmingham Daily Post*, October 4th, 1892, he said:—

"Some few years ago I began to deal with one of the most dreadful calamities to which humanity is subject by means of an operation which had been scientifically proposed nearly two hundred years ago. I mean ectopic gestation. The rationale of the proposed operation was fully explained about fifty years ago, but the whole physiology of the normal process and the pathology of the perverted one were obscured and misrepresented by a French physiologist's experiments on rabbits and dogs. Nothing was done, and at least ninety-five per cent. of the victims of this

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\* *Blue Book*, Q. 1,852.

† See *The Healing Art and the Claims of Vivisection*, by E. Berdoe, p. 37.



catastrophe were allowed to die. I went outside the experimentalists' conclusions, went back to the true science of the old pathologist and of the surgeon of 1701, and performed the operation in scores of cases with almost uniform success. My example was immediately followed throughout the world, and during the last five or six years hundreds if not thousands of women's lives have been saved, whilst for nearly forty years the simple road to this gigantic success was closed by the folly of a vivisector.

"But that is not all. One of the conclusions of my operations was a physiological one, as simple as possible, and following from my facts as certainly as night follows day. It was that the peritoneal cavity was capable of digesting the soft gelatinous tissue of an early foetus. But this did not satisfy our German men of science, one of whom immediately set to work and, removing the immature babies from the wombs of a number of animals, he planted them in the cavity of the peritoneum of the same animals. Thus he assumed that he 'confirmed' my statements. I shall not harrow your readers by a description of what the sufferings of these poor little animals must have been, because I do not take up (though I feel very keenly) what is called the mere sentiment of this question; but I proclaim that the whole of this objectionable proceeding was useless and ridiculous in its purport, for ectopic gestation is unknown in the lower animals, and therefore no conclusion derived from this German kind of science could have been of the least value. But, on the other hand, suppose that the peritoneum of these lower animals had not been capable of digesting the foetus, for animals differ vastly from man in these processes, these experiments might have created doubts as to the value of my operations on women, and the whole of this immense advance of surgery might have been stopped—a possible result really too awful to contemplate."

*Q. Did not Professor Rutherford mislead the doctors on one occasion after some very cruel experiments on the biliary secretion of the dog?*

A. Such medical men as were foolish enough to pay attention to his alleged discoveries were led into very absurd mistakes. Dr. Rutherford professed to have learned by his experiments on dogs that neither calomel nor blue pill have any beneficial effects on the liver. These drugs had long done good service in medical practice, but in consequence of these scientific experiments many medical practitioners abandoned their use.

*Q. How did the experimenter arrive at his conclusion that calomel was inactive in liver troubles?*

A. He put the drug into that part of the dog's intestines called the duodenum instead of giving it by the mouth, as it is administered in medical practice.

*Q. Would that make any difference to its action ?*

A. All the difference in the world. When we take a drug by the mouth it is mixed with the saliva and other secretions of the digestive system before it reaches the duodenum, and these secretions may exert definite chemical changes in it.

*Q. So that taking physic by the ordinary method and taking it by surgical operation is not quite the same thing ?*

A. Not by any means. After the experiments had been varied and repeated again and again, it was suggested to the experimenter that when the calomel is swallowed it gets mixed with the gastric juice and saliva which partly convert it into mercuric perchloride, commonly called corrosive sublimate.

*Q. Did that explanation clear up the muddle ?*

A. Not quite. Another fallacy complicated the matter, and that was the unnatural condition of the vivisected animals' stomachs. The dogs were all fasting and in that condition a certain mucus accumulates in their stomachs which enveloped the drugs and prevented its absorption, so that the calomel never got near the liver at all.

*Q. And what was the end of it ?*

A. Dr. Rutherford worked patiently 'on, and then found out that the doctors who gave calomel and blue pill were right after all and he was entirely wrong.

*Q. Was that view generally accepted by doctors ?*

A. It was. The Rev. S. Haughton, M.D. told the Royal Commission,\* that "the experiments have not altered our practice in the slightest degree. Even supposing a particular theory of the action of mercury on the liver is correct, it still proves that experiments on dogs have done very little for man, because they have not altered the medical practice."

*Q. What did Sir Charles Bell say about the perpetuation of error by vivisection ?*

A. He said, † "Experiments have never been the means of discovery ; and a survey of what has been attempted of late years in physiology, will prove that the opening of living

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\* *Blue Book.* Q. 1,936.

† *Nervous System of the Human Body* (Longman's, 1839), p. 217.

animals has done more to perpetuate error, than to confirm the just views taken from the study of anatomy and natural motions."

*Q. Even Professor Ferrier, although himself a notorious vivisector, has said, I believe, that experimenters have been misled by vivisection?*

*A. Yes.* He said, "One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals are capable of application to man without qualification; an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such potent differences between the effects of the destruction of the cerebral hemispheres in different orders of animals, ought to inspire caution in the application to man of results obtained only by experiments on the brains of animals low down in the scale."

*Q. I have been given to understand that in consequence of Professor Ferrier's discoveries in localisation of brain functions, the result of his numerous and prolonged experiments on animals, it is now possible for surgeons to open the heads of patients suffering from tumours of the brain and to remove them successfully.*

*A.* It is an error to assert that brain surgery and operations on the head have become possible and successful, only in consequence of experiments on living animals. Brain surgery was practised in pre-historic times. Sir John Lubbock says\* of the Society Islanders that "They had no knowledge of medicines as distinct from witchcraft; but some wonderful stories are told of their skill in surgery," and he quotes from Ellis an account of their operations on the injured brain. In the *British Medical Journal*, February 10th, 1894, p. 338, there is an account of the presentation to the Smithsonian Institution (Boston, U.S.A.), of nineteen skulls, by Dr. Munez, showing evidence of pre-historic trephining for surgical purposes. In the *Standard* for February 19th, 1894, there was a report of a lecture delivered at Toynbee Hall, London, by Professor Victor Horsley, on "Trephining in the Stone Age." The lecturer said he wished to draw attention to the fact, that the people who used to live in this country, and in France, and other parts of the Continent before the days of history, succeeded, although little better than savages of a low type, in carrying out what was now regarded as a difficult operation in surgery. He remarked that there was sufficient proof that patients survived these operations, performed something like 5,000 years ago.

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\* *Pre-historic Times*, p. 483.

Q. *What is the earliest account we have of operations on the head or brain by civilized surgeons?*

A. I am not aware that anything very definite was written on the subject before the time of Hippocrates. Of this great Father of Medicine, Dr. Adams in his *Life of Hippocrates* says, "In surgery he was a bold operator. He fearlessly, and as we would now think, in some cases unnecessarily, perforated the skull with the trepan and the trephine in injuries of the head." This was some 400 years before Christ, so that brain surgery is not a modern invention.

Q. *But localisation of brain functions is a new discovery, is it not?*

A. To a large extent it is; but this branch of our subject demands a discussion to itself.

## CHAPTER IV.

### BRAIN SURGERY, AND THE LOCALISATION OF THE FUNCTIONS OF THE BRAIN.

*Q. Admitting the extreme antiquity of Brain Surgery and operations on the skull, is it not the fact that the science of localising the functions of the brain originated with experiments on animals ?*

A. It is not the fact. In accordance with the regular practice of experimental physiologists, they ignore all research and discoveries connected with the animal organism, which have been made independently of animal experimentation. For practical purposes it may be said that Dr. François Joseph Gall, a German physician, towards the end of the eighteenth century discovered that different areas of the brain are connected with different mental and physical manifestations.\*

Dr. Gall had a distinguished pupil, Dr. Spurzheim, who joined him in 1800, and in 1810-12 they published at Paris their *Anatomy and Physiology of the Nervous System and of the Brain in particular*. Their researches led to increased study of the brain and its functions. Their system is called "Phrenology," or more correctly "Craniology."

*Q. I have always looked upon Phrenology as a pseudo-science and arrant quackery.*

A. It was and is denounced by men of science as "a purely fantastic scheme, unsupported by observation and experiment," and is treated with contempt by anatomists and physiologists.

*Q. Do they condescend to mention it ?*

A. Of late years it has been treated with rather more respect. Messrs. Landois and Stirling† say :—

"The phrenological doctrine of Gall and Spurzheim assumes that the different mental faculties are located in different parts of

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\* See "*The Wonderful Century*," by Alfred Russell Wallace, F.R.S., pp. 160, *et seq.*

† *A Text Book of Human Physiology*, 3rd Ed., 1888, p. 682.

the brain, and it is assumed that a large development of a particular organ may be detected by examining the external configuration of the head."

Q. *Then we may say that the Phrenologists located more or less accurately the mental faculties, and the physiologists mapped out the physical functions of the brain. Is that so?*

A. Approximately, yes. But Gall and Spurzheim did more than this. The early phrenologists were well aware that there was a strange communication of the muscles with the cerebral organs. Dr. Gall in one of his first writings upon his discoveries—his letter to Baron de Retzer upon *The Functions of the Brain in Man and Animals*—stated that when certain cerebral organs are put in action you are led, according to their seat, to take certain positions, as though you are drawn by a wire, so that one can discern the seat of the acting organs by the motions."

Q. *Is that generally known to medical men?*

A. I do not think it is, but Dr. Davey of Bristol, stated to the Bath and Bristol Medical Association, in 1874, that he was convinced in 1842 that the stimulation of many parts of the brain in man did excite both sensation and motion. He added:—"I affirm, that twenty-eight years before Hitzig ascertained and taught the fact as stated, the same was known to the late Dr. Elliotson, Messrs. Atkinson and Syme and others . . . Doctors Hitzig and Ferrier would not be reaping the happy harvests of their very commendable labours if things were not now altered for the better."

Q. *I have always understood that the experiments of Hitzig and Ferrier were horribly cruel, why did Dr. Davey call their labours commendable?*

A. I am afraid that the question of cruelty had little weight with the body he was addressing.

Q. *Tell me in simple language what is meant by localisation of brain functions?*

A. About the year 1870 Professor Ferrier, and several physiologists on the Continent, began to experiment on the exposed brains of living animals. They excited different spots in the brain by weak galvanic currents and observed the resulting visible effects. They discovered that where certain definite areas were excited by the current, a contraction of certain definite sets of muscles took place and these contractions caused movements of the limbs, body, face, or head of the animal. They called this Localisation of the Functions of the Brain.



Q. *Will you give me an example or two ?*

A. When Professor Ferrier excited a spot in the ascending frontal convolution of the brain of a monkey and several other animals it caused the cheeks and the angles of the mouth to be elevated and closure of the eyes. He could not obtain these effects by exciting any other part of the brain. Another centre was found which produced movements of the tongue, cheeks and jaws in monkeys. In jackals a centre was discovered which on being excited caused them to draw back the ears and spring forward. In cats, spitting and lashing the tail followed.

Q. *Then the phenomena are not the same for all classes of animals ?*

A. By no means. Mr. Alfred Russell Wallace says\* :—

“It is also very suggestive that these experiments lead to nothing of value in the hands of the experimenters. To show that the excitation of one brain-centre affects such numerous and varied sets of muscles as are required to cause movements of the hind legs, the tail, the head, and the vocal organs of a cow ; while excitation of another centre produces movements of the ear and all four limbs in a jackal, but of the tail, mouth and tongue in a not very remote species, the cat—are facts which, standing alone, are unmeaning and worthless.”

Q. *I can understand that the human brain differs immensely from that of even the highest animals, and am not surprised to hear that the brains of animals differ widely amongst themselves ; that is so, I suppose ?*

A. Yes. Dr. Radcliffe Hall says† :—

“In the rabbit, the iris receives fibres from the sixth pair [of nerves] which do not pass through the ganglion ; and it is through this that the contraction of the pupil is produced in that animal by irritation of the fifth pair, which will not produce any effect upon the pupil of the dog, cat, or pigeon, so long as it does not affect the brain to the extent of producing vertigo, nor affect the visual sense in any other way.”

Q. *Does Professor Ferrier allude to this matter ?*

A. Yes. As I have already told you, he says :—

“One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals are capable of application to man without qualification ; an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such patent differences

\* *The Wonderful Century*, p. 190.

† *Edinburgh Medical and Surgical Journal* (1846-8).

between the effects of the destruction of the cerebral hemispheres in different orders of animals ought to inspire caution in the application to man of results obtained only by experiments on the brains of animals low down in the scale."

Q. *I have heard that frogs deprived of their brains are still capable of performing a number of complicated movements, which almost seem to have been spontaneous; is that the fact?*

A. Certainly. But human beings under such conditions could not perform them.

Q. *What can a frog do whose brain has been removed?*

A. Dr. Charcot, formerly Physician to the Salpêtrière Hospital, Paris, says\*:—"It can sit, spring or execute complicated, co-ordinated movements when appropriate stimuli are applied; when placed on its back, it immediately turns into its normal position on its belly; if stimulated it gives one or two springs, and then comes to rest; when thrown into water, it swims to the margin of the vessel, and it may crawl up its side, and sit passive on the edge of the vessel." †

Q. *Are any other animals capable of such actions after losing their brains?*

A. Pigeons without their cerebral hemispheres behave in a similar manner. They are subject to impressions, but these are not perceived as conscious perceptions.

Fish behave differently. Rabbits and dogs, it is said, owing to the great loss of blood consequent on the removal of the brain, are not well suited for experiments of this kind. ‡

Q. *How have physiologists got over the difficulty of applying the results of these researches to man?*

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\* "*The Topography of the Brain*," in "*The Forum*" August, 1888 pp. 615-616.

A decapitated frog performs with its legs co-ordinated automatic movements; not so a decapitated dog. In the dog the brain lesions [structural tissue-changes from injury or disease], even of considerable extent, produce only incomplete paralysis, often passing away, while in man the like lesions cause incurable functional troubles. These examples are enough to show that, particularly as regards brain functions, the utmost reserve is necessary in drawing inferences from animals to man."

† "*A Text Book of Human Physiology*," by Landois and Stirling, 3rd Ed., p. 682.

‡ *Ibid.*, p. 683.



A. They say we must operate on the monkey as being almost akin to the human species—and they maintain that what is true of the monkey is true of man.

Q. *What do you say to that ?*

A. It is pure nonsense. In brain man differs more than in anything from the animals. In no other species does the organ of mind reach the degree of development and perfection than it attains in man. Man can speak, the monkey cannot. How could the brain centre for speech be studied in a monkey? How could it have been discovered except in man?

Q. *How was it discovered ?*

A. The investigations of Bouilland (1825), Dax (1836), Broca (1861), and others have proved that the third left frontal convolution of the brain is of essential importance for speech. In the great majority of mankind, the centre for speech is located in the left hemisphere. The vivisection of animals had nothing, and could have had nothing, to do with this great discovery. It was the outcome of clinical and pathological research. Patients suffering from paralysis of the brain affecting the right side of the body, with loss of speech, have died, and when the brains of such persons were examined after death by these investigators, it was discovered that a blood clot from the rupture of a vessel was pressing on the third left frontal convolution, or some other pathological condition interfered with its function. A long course of investigation ultimately led the faculty to locate the centre for speech in this part of the brain. This is legitimate research.

Q. *Did not Bouilland perform experiments on animals ?*

A. He did ; but he confessed that the results of his experiments were anything but satisfactory. He said that he was well aware his "propositions are at variance with the results of experiments on animals," but he knew that his bedside or clinical researches, upon which he founded his propositions, afforded the correct data for localisation of brain functions, as he not only indicated the centre for speech in the anterior lobes of the brain, but he declared that there must exist in the cerebrum several motor centres.

Q. *Has Ferrier expressed himself on the question of the value of clinical research on this subject ?*

A. He says : " Nor do the facts of experimental physiology seem so consistent with themselves or with the undoubted

facts of clinical research as to inspire us with unhesitating confidence as to their accuracy or as to their applicability to human pathology.

*Q. There are disagreements, then, amongst experimental physiologists as to the conclusions to be arrived at respecting the results of their researches?*

*A. The greatest disagreements imaginable. On another occasion we will expose them.*

## CHAPTER V.

BRAIN SURGERY, AND THE LOCALISATION OF THE  
FUNCTIONS OF THE BRAIN—(continued).

*Q. You promised to tell me about the disagreements of experimenters as to the localization of brain functions.*

*A.* After the attempts of the phrenologists to localize the mental faculties in the brain, the question entered into a new phase by the researches of the French physiologist Flourens, who lived 1794-1867. He sought by experiments to assign their special functions to the brain, *i.e.* the cerebrum, and the lesser brain or cerebellum.

*Q. To what extent did he succeed ?*

*A.* He demonstrated by experiments on pigeons to the satisfaction of his brother physiologists that the whole of the greater brain (cerebrum) is concerned in every psychical process, and that just in proportion as the grey matter of the hemispheres of the brain is removed, all the functions of the brain are weakened, and when all the grey matter is removed all the functions are abolished.\*

*Q. Did he consider that a pigeon's brain is just like that of a man ?*

*A.* The physiologists of that time did not consider such trifling distinctions as these. Many observations of the kind were made upon frogs. As we have already explained, experimenters have found that these animals can execute many complicated movements without their brains.

*Q. What view had Flourens of localization of faculties and perceptions ?*

*A.* He held that they are not localized in special areas.

*Q. Did not Goltz hold a similar opinion ?*

*A.* Yes. He assumed that if an uninjured part of the greater brain remain, it can to a certain extent perform the functions of the parts that have been removed.†

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\* *A Text-Book of Human Physiology*. Landois and Stirling, 3rd Edition, p. 682.

† Landois and Stirling, *loc. cit.*

Q. *What did Vulpian call this?*

A. He named it "the law of functional substitution."

Q. *Does not this shake the foundation of the localization theory?*

A. Naturally; but it has been much disputed.

Q. *How was Flourens supported by his brother experimenters?*

A. Majendie and his pupils by their experiments, which were very cruel, endeavoured to prove that the brain substance contains no special centres for localization of function.

Q. *What was the next step?*

A. In 1870 Fritsch and Hitzig discovered a series of circumscribed regions on the surface of what are called the convolutions of the brain, which, when stimulated by electricity, cause movements in groups of muscles on the opposite side of the body.

Q. *What do you mean by convolutions of the brain?*

A. The folding upon itself, as may be seen in a dish of raw sheep's brains. It is important to notice these either in a diagram or the organ itself, as our subject cannot be understood unless these complicated folds are recognized as the most important markings in the brain.

Q. *How do the experimenters reach the brain of a living animal?*

A. First they shave the part of the head on which they are to operate, then they remove by boring and sawing a part of the skull after they have cut through the scalp. The outer membrane of the brain called the dura mater is then divided, and the convolutions are exposed.

Q. *What do they do next?*

A. They apply the current from an electric battery to the particular region of the convolutions on which they propose to experiment.

Q. *What then?*

A. Hitzig and Fritsch announced that all that had been previously demonstrated on animals' brains was utterly wrong; that certain areas of the brains were sensitive to electricity; and that stimulus was responded to by definite movements of the different muscles of the body.

*Q. I suppose this announcement made a great stir in the physiological world?*

A. It did, and thousands of animals were subjected to the most cruel torments to prove or disprove the opposing theories of the experimenters.

*Q. I have heard that the brain is completely insensible to severe operations made upon it. Is that the fact?*

A. Experimenters say so, but I do not believe such interested evidence. The wound of the scalp, the boring of the skull, and the injuries to the membranes must cause pain. As for the brain substance itself, patients suffering from tumours of that organ certainly suffer great pain, and it is hard to believe, as physiologists maintain, that the brain is "completely insensible." At any rate, human beings are always anæsthetized thoroughly before undergoing operations of this kind.

*Q. Abscess of the brain is a painful disorder, is it not?*

A. Certainly, and if the poor animals whose brains have been scooped out, washed out, burned, galvanized, and otherwise mutilated could speak, I am sure they would not agree with their tormentors about the painlessness of their proceedings.

*Q. I suppose that experiments of this kind have never been performed upon human beings?*

A. Indeed they have. Dr. Ferrier, in his evidence before the Royal Commission,\* said: "I believe such an experiment was performed in the case of an Irish servant, who, from ulceration, had a large part of the brain exposed; and the American physician, thinking that irritation might be applied to the human brain with the same degree of safety as to the brains of the lower animals, applied electricity to ascertain whether similar movements would be induced in her as in monkeys; and he found that that was the case. The woman afterwards died, some said from the result of his experiments. I believe, however, that the woman was in a very perilous condition at the time."

*Q. How horrible. Was the woman's name discovered?*

A. She was Mary Rafferty, a servant maid. In the course of the experiments needles were thrust through the dura mater into the substance of the brain, which was then stimulated by electric shocks.

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\* *Report of Royal Commission on Vivisection, Q. 3390, p. 176.*

*Q. Are there any other such cases on record?*

A. Oh yes. Dr. Ezio Sciamanna experimented on the brain of an Italian named Rinalducci, who was admitted into the hospital at Turin on March 23rd, 1882, suffering from fracture of the skull. He underwent the operation of trepanning which left a portion of the surface of the brain exposed. The electric current was applied to the brain and was so intense that it was felt to the ends of the fingers. The patient was not anæsthetised.\*

*Q. Tell me what motor areas Hitzig and Fritsch mapped out?*

A. In 1870 they made a map of the brain of the dog, marking out the region which governs the movements of the muscles of the neck; of the movements of the fore-limb; of the movements of the hind limb, and of the muscles of the face.

*Q. Was that all?*

A. Then in 1873 Ferrier mapped out the following regions in addition, that of the movements of the tail; another region for additional movements of the fore limb; one for raising the shoulder, etc.; another for closing the eyelids, and so forth.

*Q. Were not these localizations of functions generally accepted by physiologists?*

A. By no means, notwithstanding the terrible and cruel character of the experiments on animals sacrificed to determine them. At the International Medical Congress held in London in 1881 a famous discussion took place between Dr. Ferrier and Professor Goltz, a notorious German experimenter. The transactions of this Congress were published in full, and in Goltz's address we read as follows:—

"Flourens thought that all parts of the cerebrum subserved the same functions. Fritsch and Hitzig considered it to be proved that the cortex of the cerebrum (the convolutions) may be laid out in circumscribed regions or centres, each of which presides over a special function. The investigations of Fritsch and Hitzig were continued first by Ferrier, and afterwards by a number of other experimenters. I will not here enter into the details of their observations, because I only wish to insist on the most important differences between them. The hypotheses of all these investigators have so much in common, that they divide the cortex of the brain into small circumscribed spaces, centres, or spheres, to each of

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\* *Zoophilist*, December, 1883, p. 310-313.

which they ascribe a particular function. In the limitation and position of these spaces, and in the functions attributed to them, we find the most remarkable contradictions between different experimenters, and especially between Ferrier and Munk. Notwithstanding these contradictions, the medical public accepted with acclamation these modern discoveries of localization. The writers of a number of new manuals waxed enthusiastic over them, whereas some hesitation should have been felt in accusing so clear-headed a man as Flourens of such glaring errors of observation. The reason that the new doctrines were so credulously received was evidently that the ground was ready for them, and that pathological observation had led up to the need of separation of the functions of the brain."\*

*Q. Oh, I see, pathological observation, not experiments on animals suggested the idea of localization of functions.*

A. Goltz admits it, but do not interrupt, we shall come to that presently. He continues—

"There was a fascination in the precision of the new teaching, and in the apparent agreement between the results of stimulation and destruction. A fruit may, however, appear very tempting, and yet be quite worm-eaten at the core, and it is not difficult to show the worm-eaten place in all the localization hypotheses that have yet been put forward."

*Q. What reply did Ferrier make to all this?*

A. He said he differed widely from Goltz and rejected his conclusions. "It seems to me," said Dr. Ferrier, "a matter of essential importance that in generalizing as to the functions of the cerebral hemispheres, we should be careful lest the hypothesis we adopt, however well it may seem to accord with the facts of experiment on one order of animals, should not stand in flagrant contradiction to facts, equally well established, obtained by experiments on others."

*Q. This is a point to remember. If I understand it correctly, the rabbit's brain is not the same as the dog's, nor the dog's as the monkey's; still less is the monkey's like our own. Is this so?*

A. Certainly. Dr. Ferrier said, "In the frog, pigeon, rabbit, etc., the removal of the cerebral hemispheres produces comparatively slight and transient paralysis; whereas, in the monkey and man the paralysis is permanent."

*Q. Did any other physiologists take part in the discussion?*

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\* "The Topography of the Brain" in "*The Forum*" (New York), August, 1888, p. 615-616

A. Several did. Professor Gerald Yeo said, "The first point that strikes one is that these very remarkable negative results obtained by my friend, Professor Goltz, in the case of dogs, cannot be said to be an adequate argument against the positive results arrived at in our experiments upon monkeys; while on the other hand, our positive results seem to curtail in an absolute manner the very extensive generalizations Professor Goltz wishes to draw from his experiments."

Q. *Were not some mutilated animals from the physiological laboratories exhibited on this occasion?*

A. Yes, a dog and a monkey, and in the first volume of the *Transactions of the International Medical Congress* of 1881, from which we have quoted, there are engravings showing the terrible injuries of the dog's skull and its severely mutilated brain, which were exhibited by Professor Goltz, and one of the brains of Dr. Ferrier's monkey.

Q. *Has Professor Charcot, who was, I believe, an authority on the brain of the highest rank, expressed himself on these localization theories?*

A. Yes. He has published a good deal on the subject. In one of his lectures he said, "We must not forget that it is man we are considering—man who relatively to the functions of his higher nervous centres is so far removed, in many points, from even animals highest in the scale." In another place he says, "In brain it is, above all, that we differ from animals. That organ attains in man a degree of development and of perfection not reached in any other species."



## CHAPTER VI.

### LOCALIZATION OF THE FUNCTIONS OF THE BRAIN AS AN AID TO SURGERY.

*Q. Now I want to learn all you can tell me as to the value of these brain experiments in human surgery?*

*A. There is an immense mass of expert evidence to the effect that the value of this branch of research to practical surgery is greatly exaggerated.*

*Q. Has Professor Ferrier made any admissions to this effect?*

*A. Yes, and of the most instructive character. At the meeting of the British Medical Association, in Edinburgh, in 1898, he said:—"The treatment of intracranial tumours forms rather a melancholy chapter in therapeutics. . . . The new-born enthusiasm of some ten years ago had rather cooled down of late; this is in great measure a reaction from its too indiscriminate application, and under-estimation of the risks inseparable from what in all circumstances must be regarded as a serious undertaking."\**

*Q. Did he give any reasons for this statement?*

*A. Dr. Ferrier went on to say that:—*

*"Unfortunately for those so afflicted, the great majority of cerebral tumours are so situated, or of such a nature, as to preclude the possibility of their successful removal. . . . It is fair to say that only seven per cent. of cerebral tumours are capable of being surgically dealt with. . . . At best it is evident that the proportion of cases of cerebral tumour in which there is a reasonable prospect of successful surgical interference is a comparatively small one. This is a conclusion much to be deplored, but we must accept the facts, and recognize the limitations which they place on our efforts."*

*Q. But surely many tumours of the brain have been successfully removed?*

*A. It depends on what you mean by "successfully." Dr. Ferrier added that:—*

*"Even when the tumour has been found, and successfully*

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*\* British Medical Journal (October 1, 1898, p. 664).*

removed in the surgical sense, this does not imply that the patient has recovered."

"Successful in the surgical sense," is excellent, but it is not inconsistent with sudden death.

Q. *How would you estimate the success of such an operation?*

A. Dr. Ferrier thinks that a patient after an operation should live a while if the case is to be considered "successful." He regrets that information on this head is largely absent from surgical statistics, "which are thus rendered in many respects unreliable."

Q. *To what do you attribute the failures?*

A. At the Medical Congress, recently held in Moscow, Dr. Oppenheim of Berlin, read a paper on "The Failures in Brain Surgery because of Faulty Diagnosis," in which he said that "brain localization is often most doubtful; the diagnosis of brain abscess from brain tumour is often almost if not quite impossible." He declared that the symptoms are frequently so deceitful, that it is not possible even to discover on which side of the brain the tumour should be looked for.

Q. *Was his opinion confirmed by any surgeon of eminence?*

A. Yes. Dr. Bergmann, of Berlin, also read a paper on "Brain Surgery," in which he said that the localization of functions was not enough to guard the surgeon from error; he had himself opened the skull for tumour and found none.

Q. *Have you had any experience of these failures in your practice?*

A. Yes. I once sent a poor man into hospital, who was suffering from softening of the brain, a disease in which an operation is out of the question. However, the localizers of brain functions in the hospital diagnosed a tumour. They opened the man's head, but found neither tumour nor abscess. The man promptly died. A *post-mortem* examination was made, and my diagnosis was confirmed; the man suffered from softening of the brain and that only.

Q. *Are not operations on the brain frequently undertaken for the cure of epilepsy?*

A. Yes, but the operations are generally, if not always, failures. Mr. William Thorburn, F.R.C.S., writer of the article on "The Surgery of the Brain," in the *Medical Annual* for 1897, says:—"Unfortunately, we find in the work of the last twelve months no evidence of improvement in the results

obtained by late operation after epilepsy has set in. . . . There is a want of proof that epilepsy has been permanently cured."

*Q. What does Mr. Thorburn say about the removal of tumour of the brain ?*

A. He says—"Roughly speaking, the operation failed to reveal the expected tumour in one quarter of all the cases; the mortality amounts to about another quarter, and in only about half the cases was a tumour removed without a fatal result . . . and of the seventy-two cases which 'recovered' many were doubtless rendered more or less paralytic, while in others recurrence will have ensued." \*

*Q. What do you think of operations on the head for the relief of idiocy and imbecility ?*

A. I will answer in the words of Mr. Thorburn. "The general tendency of recent literature is to condemn an operation which rests upon so uncertain a scientific basis as an alternative action upon the brain or an educational influence upon the imbecile." †

*Q. Do you condemn all operations on the head and brain ?*

A. By no means. I am only disproving the contention of the Vivisectors that by experiments on animals surgeons have learned to perform successful operations on their patients, which they could not have undertaken before the discovery of the localisation of brain functions by Ferrier and others.

*Q. What was done in injuries to the head causing convulsions, epilepsy, etc., before the localisation of functions theories were broached ?*

A. Surgeons examined the head carefully for depressions of the skull caused by blows or falls, and trephined over the seat of injury, and by raising the bone which, being depressed, was pressing upon the brain, relieved the symptoms and cured the patient.

*Q. Do not the Vivisectors say that the Anti-vivisectionists would have no right to localise brain disease or remove cerebral tumours,*

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\* *The Medical Annual*, 1897 ; Art., "Brain," p. 147.

† *Ibid.*, p. 149.

*because all that knowledge has come to us from the researches of Ferrier and Horsley?*

A. They do, but this is as impertinent as it is false. We shall avail ourselves of all the knowledge of brain surgery given us by surgeons, from Hippocrates to Hughlings-Jackson and Charcot, whose researches were made at the bedside and *post-mortem* table, and had no reference to experiments on animals.

## CHAPTER VII.

### THE FUNCTIONS OF THE SPINAL NERVES.

*Q. It is commonly stated that the functions of the roots of the spinal nerves were discovered by experiments on living animals—Is that so?*

A. It is not. On the contrary, the great discovery, second only to that of Harvey and the circulation of the blood, was made by Sir Charles Bell by a chain of reasoning of a highly philosophical character, and he has declared in the most emphatic terms possible that his discoveries were deductions from anatomy, and when he had recourse to experiments, he did so not to form his own opinions but to impress them upon others.

*Q. What is the difference between Anatomy and Vivisection?*

A. Anatomy is the dissection of a dead body, Vivisection the cutting into a living animal.

*Q. I do not quite understand what you mean by the functions of the roots of the spinal nerves?*

A. In a loin of mutton, a sirloin, or rib of beef, we often see an uninjured piece of the spinal cord or marrow. Now this spinal cord gives off large nerves right and left throughout its whole length. We call these spinal nerves. Each nerve has a posterior root and an anterior root which unite to form a spinal nerve trunk. Sir Charles Bell discovered, as I explained, that each of these roots has its own function, the posterior root is a sensory nerve and the anterior a motor nerve.

*Q. I have heard that this can be proved by experiment?*

A. Doubtless. For example, in a popular school book, entitled *Human Physiology*, by William S. Furneaux, we are told (p. 188) that—

“The functions of the roots of the spinal nerves have been ascertained by experiments. If the spinal canal of an animal be laid open, and the anterior roots of the spinal nerves supplying a certain limb be divided the animal will lose all power of voluntary movement in that limb, which will hang in a flaccid condition, but the power of sensation in the limb will remain unimpaired. If,

now, those ends of the cut roots which remain in contact with the cord be irritated no effect is produced ; that is the animal will show no sign of pain, neither will there be any movement of the limb. But if the other ends be irritated, the muscles of the limb will contract violently. Thus we learn that the anterior roots consist of efferent or motor fibres only. If the posterior roots supplying a certain limb be divided without injuring the anterior roots, the animal will still have control over the voluntary muscles of that limb just as if nothing had happened, but the limb may be pinched, or even burnt, without producing any sign of suffering. Again, if those ends of the posterior roots still in contact with the cord be irritated, the animal will exhibit signs of pain ; but no effect is produced by the irritation of the other ends. Therefore we conclude that the posterior roots consist of sensory or afferent fibres only."

Q. *Such experiments must involve awful suffering, because anæsthetics would naturally be out of the question in cases involving researches in pain, I presume ?*

A. Of cou se. It is important to remember that in all cases where the study of the production or the action of pain is the object of the research, any attempt at anæsthesia would frustrate the purpose of the experimenter.

Q. *Have cruel experiments been performed for the purpose of studying the action of pain ?*

A. Certainly. Here is an example. In the *Lancet*, March 25th, 1871, p. 415, we find the following :—

"The *Gazetta Italiana di Milano* contains an essay of Professor Mantegazza on experiments carried on under his direction at the Laboratory of Experimental Pathology of the University of Pavia. It will suffice to state that the experiments were intended to study the action of pain on digestion and nutrition. They were, as the Professor himself confesses, agonizing to the animals subjected to them, and distressing to the experimenters, and simply proved that loss of appetite, great weakness, and a peculiar inhibition of moisture were the result of the pain inflicted. It is added that no alteration of the spinal marrow could be detected after the agony had been protracted for one month. Very meagre results of unpardonable cruelty."

Q. *I see how experiments on living animals would serve to demonstrate the functions of the roots of the spinal nerves. What I want to be clear about is how Sir Charles Bell made his discovery ?*

A. You are right to ask this question as there is all the difference in the world between a discovery and its subsequent demonstration. Sir Charles Bell says,\* "Experiments have never been the means of discovery."

\* *Nervous System of the Human Body* (Longmans & Co., 1839), p. 127.

*Q. This seems at variance with all we know of scientific advance?*

A. Not at all! The true discoverer is a man of genius, he is inspired like the poet. Read what Professor Tyndall says of the scientific use of the imagination: "He [the discoverer] lives a life of the senses, using his hands, eyes and ears in his experiments, but is constantly being carried beyond the margin of the senses. His mind must realise the subsensible world, and possess a pictorial power."

*Q. Where precisely did Sir Charles Bell's experiments come in?*

A. They were only performed upon animals subsequently to his discovery. They were made for the sole purpose of convincing those who were sceptical. His discoveries were obtained by pathological observations combined with an accurate knowledge of human and comparative anatomy and a capacity for acute reasoning.\*

*Q. Can you give me the exact words of Bell about his discovery?*

A. Certainly. He says:—†

"In concluding these papers, I hope I may be permitted to offer a few words in favour of anatomy, as better adapted for discovery than experiment. Anatomy is already looked upon with prejudice by the thoughtless and ignorant; let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has been done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions. In a foreign review of my former papers the results have been considered as a further proof in favour of experiments. They are, on the contrary, deductions from anatomy, and I have had recourse to experiments, not to form my own opinions, but to impress them upon others. It must be my apology that my utmost efforts of persuasion were lost until I urged my statements on the grounds of anatomy alone."

*Q. Nothing could be plainer than this, yet I have often heard apologists of vivisection declare that Bell was a vivisector and used living animals in his researches on the nerves.*

A. In one sense this is true, but it is not true in the sense the vivisectors use the argument. We must insist on the

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\* See the question fully discussed in Dr. Wall's Prize Essay, "Painful Experiments on Living Animals Scientifically and Ethically Considered," p. 264 *et seq.*

† *Nervous System of the Human Body* (1839), p. 217.

distinction Bell himself has made between the discovery and its subsequent demonstration to his scientific opponents.

*Q. Since our last conversation I have listened to a debate on Vivisection, conducted by barristers and doctors, and it was maintained that notwithstanding Sir Charles Bell's emphatic protest, his experiments on animals really led to his discoveries on the functions of the nerves. Can you explain this a little more?*

*A. It is disgraceful of the vivisectioners to traduce the memory of this great surgeon; but it shows that they will stick at nothing rather than concede his axiom that "experiments have never been the means of discovery." They cannot afford to abandon their claim on Bell's and Harvey's discoveries, although they have not the least right to either.*

*Q. You said at our last meeting that Bell made experiments on animals after his discovery, that he might convince his sceptical brethren. Surely this was useful?*

*A. Of course it was. But we are discussing Bell's discovery, and not its acceptance by other surgeons.*

*Q. But I have often heard lecturers declare that experiments on animals are utterly useless.*

*A. Useless to practical medicine, and so relatively useless; but not useless to abstract science. Nothing is useless that tends to increase the sum of human knowledge. Every discovery, however unimportant it may seem to the practical man to-day, may in the future be of the greatest importance.*

*Q. I have heard that many experiments of the greatest utility have been performed on bodies of animals recently dead, by means of electricity. Is this correct?*

*A. Certainly it is. MM. Longet and Mattenci, in alluding to the different and remarkable action of electric currents on nerves, say that this method appears to "contribute a sure means of distinguishing these nerves from each other, and consequently serves to elucidate a question which has hitherto divided physiologists."\**

*Q. Are not many experiments on the nerves carried out on decapitated or pithed frogs and other animals?*

*A. Yes. For example, in connection with what are called "protective movements." If a drop of dilute acid be applied to the skin of a frog whose head has been cut off, or whose brain has been destroyed, the creature although dead*

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\* "Vivisection. Prize Essays," p. 263.



so far as any sensation is concerned, immediately strives to get rid of the offending body, and it generally succeeds in doing so.\*

Q. *If the animal cannot feel, how is it these purposive acts are performed?*

A. Pflüger regarded them as directed by, and due to, "consciousness of the spinal cord." A pithed frog croaks every time the skin of its back or flanks is gently stroked.

Q. *These are very important facts. Do they not explain some of the cases in which it is claimed that vivisection has aided physiological research?*

A. They do. Professor Michael Foster wrote the article "Physiology" in the last edition of the *Encyclopædia Britannica*, and in defending the practice of vivisection he points with triumph to the discovery of the inhibitory functions of certain nerves by means of animal experimentation.

Q. *What do you mean by inhibition?*

A. The restraint of the activity of an organ from nerve action. This was a very important discovery no doubt, but the professor did not tell his readers that the experiments on the nerves of the heart have for the most part been carried out on the frog, and that a "pithed" frog, that is to say a dead one. Messrs. Landois and Stirling say † that "Schiff found that stimulation of the vagus nerve of the frog caused acceleration of the heart beat when he displaced the blood of the heart with saline solution. If blood-serum be supplied to the heart the vagus regains its inhibitory action." Again, on p. 673 the authors say: "If two frogs be pithed and their hearts exposed, and both be suspended, then the hearts of both will be found to beat rhythmically and fill with blood. Destroy the medulla oblongata and spinal cord of one of them, then immediately in this case the heart, although continuing to beat with an altered rhythm, ceases to be filled with blood," and so on.

Q. *You do not object, of course, to this sort of experiment?*

A. Of course not. We only object to torture. These frogs are dead, but as the heart of a frog continues to beat long after its removal from the body, it is a legitimate subject of experimentation. It is not vivisection, and it is not honest to quote discoveries made in this manner as though they had been made on living animals.

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\* *A Text Book of Human Physiology.* Landois and Stirling—3rd Edition, p. 638.

† *Ibid.*, p. 669.

## CHAPTER VIII.

### THE OPERATION FOR TREPHINING THE SPINE.

*Q. A friend of mine suffering some years ago from a tumour on the spinal cord which caused intense pain for many months, underwent an operation for its removal by a distinguished surgeon, who is also a notorious vivisector. The operation was successful, the paralysis was cured, and the pain gradually ceased. The operator declared that without previously satisfying himself by experiments on monkeys that the operation was feasible, he would not have ventured to open the spinal canal in the human subject. What have you to say to this?*

*A. I am glad you have adduced something definite like this. It is seldom that it happens that opponents of vivisection can induce their doctors to specify in set terms exactly what they mean by the utility of vivisection in connection with the Healing Art.*

*Q. Do you know that the whole case is recorded in the "Transactions of the Royal Medical and Chirurgical Society" for 1888, by Dr. Gowers and Mr. Horsley?*

*A. Yes, I remember it perfectly well, and I may say that after careful reading I can find no reference to the monkeys at all.*

*Q. Possibly the reference to the monkeys was a private communication to the patient, and was not referred to in the medical report itself.*

*A. No doubt that was so. Vivisectors often talk like this when they are not liable to be contradicted.*

*Q. What is the true history of the operation?*

*A. You shall have it in the words of Professor Erichsen.\**

*"As the fatal result of fracture of the spine is almost inevitable, and as it is undoubtedly dependent upon the injury sustained by the cord from its compression or division by the broken vertebrae, the idea has naturally suggested itself to surgeons that life might*

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*\* Science and Art of Surgery, Vol. I., p. 411, 1869.*

be prolonged, and health perhaps restored if the same operation were extended to the spine, which is successfully employed in parallel cases of injury to the head; viz., the elevation and removal, if necessary, of the depressed portion of bone."

*Q. I see. The operation for trephining the skull to relieve pressure on the brain suggested that on the spine to relieve pressure on the cord, whether from depressed bone or a tumour. It was a process of reasoning, not the happy result of cutting into an animal.*

A. Certainly. In disease the spinal bones are often destroyed, and in gun shot-wounds are frequently injured. If the spinal cord itself escapes there is no reason why the patient should not do well.

*Q. Who first performed the operation of trephining the spine?*

A. The operation was originally proposed by Heister, and performed by Louis and Cline. At the date of the publication of the 5th edition of Erichsen's *Surgery* (1869) permanent success had only been obtained in one instance by Dr. Gordon, of Whitworth Hospital, Dublin. Mr. Timothy Holmes\* says that the operation was approved by Celsus.

*Q. Do any authorities refer to experiments on animals in this connection?*

A. Mr. Holmes, in the *System of Surgery* already referred to, quotes Professor Gwilt to the effect that no light could be derived from experiments on animals because trephining the spine with them is a very different thing from such an operation in a human being. Professor Gwilt says that the results obtained from experiments on animals are not in harmony with the clinical or pathological histories of the cases in which the operation in the spine has been performed in the human patient.

*Q. Does not the operation for removing a tumour from the spinal cord differ somewhat from the mere removal of pressure as from depressed bone?*

A. Of course. And Mr. Horsley's was the first case in which a tumour involving the spinal cord had been exposed and removed. He is entitled to all the honour due to so successful an operation. But inasmuch, as in his own words we are told that "this is the first case" in which such an operation was performed, it follows that it could not have been done in consequence of similar operations on monkeys or other animals.

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\* *System of Surgery*, Vol. I., p. 673.

*Q. Naturally. But had animals anything to do with the case?*

A. Mr. Horsley claimed that the question of using blunt or sharp instruments was settled by experimenting on dogs. There is the story of a very pretty quarrel in this matter between Mr. Horsley and Professor Schiff recorded in *Brain*, Vol. IX., 1886. A number of dogs were trephined in the spine by Mr. Horsley, and then Professor Schiff tells of the injuries which he inflicted on the brains of more than one hundred puppies. Each experimenter accuses the other of having been led astray in this researches, and the whole controversy between these two vivisectors is a bitter satire on the practice of Vivisection as a method of Medical research.

## CHAPTER IX.

## THE NERVES AND THEIR FUNCTIONS.

*Q. You explained that Sir Charles Bell discovered that the anterior roots of the spinal nerves are motor and the posterior are sensory. Are there any nerves with mixed functions?*

*A. There are. The vagus or pneumogastric nerve has both sensory and motor fibres. The superior laryngeal nerve which arises from the pneumogastric and is distributed to the larynx is a nerve whose functions are those of sensation and motion. The same is true of the cervical nerves and of many others.*

*Q. Explain what is meant by "Recurrent Sensibility."*

*A. Magendie discovered that sensory fibres are also present in the anterior or motor roots so that their stimulation causes pain. This is due to the fact that sensory fibres pass into the anterior root after the two roots have joined, and these fibres run in the anterior root in a centripetal direction. The sensibility of the anterior root is abolished at once by section of the posterior root. This condition is called "recurrent sensibility" of the anterior root.\**

*Q. What are the nerves of the nerves or Nervi Nervorum as you call them?*

*A. It has been discovered that the nerve-sheaths are provided with special nerve fibres in virtue of which they are endowed with sensibility; the nerves, both sensory and motor, have therefore their own sensory nerves.†*

*Q. I was anxious to have this made clear, because I have often been told when I have mentioned the stimulation of nerves by electricity in vivisectioned animals, that it is only the motor nerves which are so treated, and that it causes the animal no pain.*

*A. This is wrong. The sympathetic nerve has very complex functions. "The general processes which it appears to influence are those of involuntary motion, secretion and*

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\* Landois and Stirling's *Physiology*, p. 617, 3rd Edition.

† *Ibid.*, p. 617.

nutrition.”\* Though it is a nerve of motion its fibres conduct impressions, as when any of the viscera usually unfelt give rise to sensations of pain. As Kirkes says, “So, also, in experiments, now more than sufficiently numerous, irritations of the semilunar ganglia, the splanchnic nerves, the thoracic, hepatic, and other ganglia and nerves, have elicited expressions of pain, and have excited movements in the muscular organs supplied from the irritated part.”† The splanchnic nerve, for example, has *motor fibres*, is the *vaso-motor nerve* of the blood-vessels of the intestines, and at the same time is the *sensory nerve* of the intestine, and, under certain circumstances, it may give rise to very painful sensations.

Q. *There must be great difficulty and many sources of fallacy in experimenting on the nervous system of animals if this be the case?*

A. Certainly. Dr. Reid, who performed many experiments on the vagus or pneumogastric nerve, said, “The experimental history of the par vagum furnishes an excellent illustration of the numerous difficulties with which the physiologist has to contend, from the *impossibility* of insulating any individual organ from its mutual actions and re-actions when he wishes to examine the order and dependence of its phenomena.”

Q. *But are not such experiments valuable aids to medical research even if at times they seem to yield contradictory results?*

A. I have no confidence in them, because it is evident that the principal reason for their performance is to contradict the results of previous workers in the same field of research. In physiology one can only hope to reach a higher rung of the ladder by pulling down the man above you. Says Dr. Wall,† “It would be amusing, were it not lamentable, to see physiologists at every point quarrelling among themselves as to the true interpretation of experiments vitiated by the induced functional disturbance, and beyond the power of reason to fathom or unravel. In this experiment, as in others, it is the difficulty of overcoming or estimating aright the amount of systemic disturbance that causes this divergence of opinion; and whilst such sources of fallacy exist, it is impossible to attach any value or significance whatever to such results as these; they simply leave us on the horns of a dilemma, and consequently the danger of applying any of the results in practice is obvious.”

Q. *I see; it is life—that mysterious element—which makes all the difficulty.*

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\* Kirkes's *Handbook of Physiology*, 8th Edition, p. 573.

† *Ibid.*, p. 572.

‡ *Painful Experiments, etc.*, p. 268.

A. Exactly. Physiologists treat their researches on organic processes as if only one operation, one experiment, were being conducted in the laboratory, whereas it is as though while they were engaged in one definite experiment a thousand others were being conducted by invisible agents in every square foot of their work-room, every one of which had a greater or less influence on the work they were performing.

Q. *The persistence of vitality being an obstacle to accurate reasoning from experiments, would it be of any use to experiment on dead bodies?*

A. Certainly, and many most important results have been attained by that method. For example, Volkman tied a glass tube, drawn fine at one end, into the windpipe of a beheaded animal, and when the small end was turned to the flame of a candle, he galvanized the pneumogastric nerve. Each time he did so the flame was blown, and once it was blown out. This experiment, which was perfectly legitimate, demonstrated an important point in the phenomena and treatment of spasmodic asthma.

## CHAPTER X.

### THE CIRCULATION OF THE BLOOD.

Q. *How was the circulation of the blood discovered ?*

A. I may premise that the ancients knew nothing of the circulation of the blood, although they performed numberless experiments on the living bodies of men and animals. Herophilus, of Chalcedon, one of the most famous physicians of the ancient world, founded with other doctors the famous Alexandrian School of Medicine. He is said to have dissected alive as many as six hundred criminals.

Q. *Did he make many discoveries ?*

A. He made many, and those of the highest importance. He founded the doctrine of the pulse, that it is communicated by the heart to the walls of the arteries. He distinguished between arteries and veins, and admitted that the arteries contain blood though it was believed at the time that they contain air.

Q. *He should certainly not have missed making the discovery of the circulation if vivisection could have assisted him.*

A. It would be impossible to discover it by such means if it had to be done now. Mr. Lawson Tait, the distinguished surgeon, says,\* "It is . . . perfectly clear that were it incumbent on any one to prove the circulation of the blood now as a new theme, it could not be done by any vivisectional process, but could at once be satisfactorily established by a dead body and an injecting syringe."

Q. *It is sometimes declared that quite as much as Harvey knew was known before his time, and that it is only our insular pride which has attributed the discovery to him. What are the facts ?*

A. Nemesius, Bishop of Emissa (near the end of the fourth century), wrote a treatise on the *Nature of Man*, in which he described the circulation of the blood in very plain terms considering the state of physiology at that time. He said, "The motion of the pulse takes its rise from the heart, and

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\* *The Uselessness of Vivisection*, pp. 3, 4.



chiefly from the left ventricle of it; the artery is with great vehemence, dilated and contracted by a sort of constant harmony and order."<sup>\*</sup>

Q. *What was the next step?*

A. Mondino, the anatomist of Bologna, who dissected and taught in 1315, had some idea of the circulation of the blood, for he says that the heart transmits blood to the lungs.†

Q. *Who were Harvey's immediate predecessors in this research?*

A. Servetus, Realdus Columbus, Sylvius, Fabricius and Cæsalpinus. We will briefly consider what each of these anatomists contributed to the great discovery. Servetus, who was burnt by Calvin in 1553, was the first person who distinctly described the small circulation, or that which carries the blood from the heart to the lungs and back again to the heart. Servetus was certainly the true predecessor of Harvey in physiology, this is universally admitted.‡ Some writers consider that Columbus had a still greater share than Servetus in the discovery. He correctly taught that the blood passes from the right to the left ventricle, not through the partition in the heart, but through the lungs. Harvey quotes Columbus, but does not mention Servetus.

Q. *What was the chief anatomical discovery about the blood-vessels which preceded that of the circulation?*

A. That of the discovery of the valves of the veins, their existence was found out by Sylvius, but Fabricius remarked that they were all turned towards the heart. Now Harvey was a pupil of Fabricius (1604), and having learned from him the full meaning of the pulmonary circulation the way was paved for the demonstration of the complete circulation. Cæsalpinus is another claimant for the honours accorded to Harvey. He first named the blood flow "circulatio."

Q. *What had vivisection to do with Harvey's researches?*

A. He began his investigations by dissecting a great number of living animals, dogs, pigs, serpents, frogs, and fishes. He observed and experimented upon the auricles, the ventricles, the arteries, and the veins. Above all, he learned that the object of the valves in the veins is to favour the flow

\* See Friend, *Historia Medicinæ*, p. 833.

† Whewell, *Hist. Induct. Sciences*, Vol. iii., p. 394.

‡ See *Ency. Brit.*, art. "Harvey."

of the blood towards the heart; and it was to this observation, and not to the vivisection, that he himself attributed his discovery.

Q. *Can you prove this?*

A. Certainly. "I remember," says Boyle, "that when I asked our famous Harvey what were the things that induced him to think of a circulation of the blood, he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave a free passage to the blood towards the heart, but opposed the passage of the venous blood the contrary way, he was incited to imagine that so provident a cause as Nature had not placed so many valves without design; and no design seemed more probable than that the blood should be sent through the arteries, and return through the veins, whose valves did not oppose its course that way."

Q. *What does Harvey himself say about the matter?*

A. In the first chapter of his famous book *On the Motion of the Heart and Blood in Animals*, he says, "When I first gave my mind to vivisection as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection, and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Frascatorius, that the motion of the heart was only to be comprehended by God."\*

Q. *You do not deny that Harvey vivisected?*

A. Of course not. What I am proving is that the main result of his experiments on living animals was to mystify him. We maintain on the authority of his own book that it was anatomy and reasoning therefrom that led him to his great discovery.

Q. *Give me the exact words.*

A. He said †: "And sooth to say, when I surveyed my mass of evidence, whether derived from vivisections, and my various reflections on them, or from the study of the ventricles of the heart, and the vessels that enter into and issue from them, the symmetry and size of these conduits,—for nature doing nothing in vain, would never

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\* *Harvey on the Circulation of the Blood.* Dr. Bowies' Edition, p. 20.

† *Ibid*, p. 48.

have given them so large a relative size without a purpose,—or from observing the arrangement and intimate structure of the valves in particular, and of the other parts of the heart in general, with many things besides, I frequently and seriously bethought me, and long revolved in my mind, what might be the quantity of blood which was transmitted; in how short a time its passage might be effected, and the like. But not finding it possible that this could be supplied by the juices of the ingested aliment, without the veins on the one hand becoming drained, and the arterties on the other getting ruptured through the excessive charge of blood, unless the blood should somehow find its way from the arteries into the veins, and so return to the right side of the heart; I BEGAN TO THINK WHETHER THERE MIGHT NOT BE A MOTION, AS IT WERE, IN A CIRCLE. Now this I afterwards found to be true; and I finally saw that the blood, forced by the action of the left ventricle into the arteries, was distributed to the body at large, and its several parts, in the same manner as it is sent through the lungs, impelled by the right ventricle into the pulmonary artery, and that it then passed through the veins, and along the *vena cava*, and so round to the left ventricle in the manner already indicated."

*Q. He says that part of his evidence was derived from vivisections.*

A. He does, but when he goes on to explain the grounds which led him to form his opinion he mentions nothing which was not the outcome of the study of anatomy and reasoning on what his dissection of the dead body led to. Thus he mentions "the study of the ventricles of the heart and the vessels that enter into and issue from them"—this was anatomy and could not have had anything to do with vivisection. Then he refers to the size and symmetry of the blood vessels—anatomy again and not vivisection. "The arrangement and intimate structure of the valves"—again anatomy, and anatomy alone, vivisection could not have helped in the least.

*Q. But he says, "I finally saw that the blood, forced by the action of the left ventricle into the arteries, &c."*

A. Yes, but by "saw" he plainly meant "understood," it was not the eye but the mind that was at work. As a matter of fact he could not have seen this with his bodily eyes. Dr. George Macilwain, a witness before the Royal Commission on Vivisection, said, "I find that the discovery of the circulation of the blood is referred to vivisection. In the first place, any man who knows what the circulation is will see that

intrinsically that could not be; you do not want the authority which is suggested to you, because you could not discover the circulation in the living body; I do not see how it is possible to do it. If you had a dead body, then it is so easy to discover the circulation that it is difficult to understand how it was not done before; because if you inject by the arteries you find that it is returned by the veins.”\*

Q. *Some persons maintain that at most Harvey discovered only part of the circulation. What do you say to that?*

A. Strictly speaking, it is true. The circulation was not proved till Malpighi used the microscope, then the visible demonstration was completed.

Q. *When was that?*

A. In 1661 this celebrated anatomist saw in the lungs of a frog, by the aid of the newly invented microscope, the blood passing from one set of vessels to the other.

Q. *Was that the capillary circulation?*

A. Yes. He traced the termination of the arteries in the veins, and so the whole circle was completed. Harvey demonstrated the circulation in the period 1616—1619, and published his discovery in 1628.

Q. *You say Harvey demonstrated the circulation?*

A. Yes, but Dr. Willis, his biographer, admits that “he left the doctrine of the circulation as an inference or induction only, not as a sensible demonstration. He adduced certain circumstances, and quoted various anatomical facts, which made a continuous transit of the blood from the arteries into the veins, from the veins into the arteries, a necessary consequence; but he never saw this transit; his idea of the way in which it was accomplished was even defective; he had no notion of the one order of sanguiferous vessels ending by uninterrupted continuity, or by an intermediate vascular network in the other order.”

Q. *Did Malpighi vivisect?*

A. Yes, but his proceeding was wholly unnecessary, for he could have better and much more easily have used the web of the frog’s foot than its lung.

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\* *Evidence Royal Commission.* (London, 1876. Q. 1845-6.)



*Q. The circulation is now demonstrated, I believe, by looking at the web of a frog's foot under the microscope, and that does not entail any cruelty?*

A. No. Merely some inconvenience to the animal by its being fixed to the frog plate.

*Q. Can you tell me what influence this discovery has had on practical medicine?*

A. It is often said that the operation of "transfusion," or the transfer of blood from one animal into the veins of another is due to Harvey's discovery, but this is not the fact. Paracelsus, who lived 1493-1541, anticipated our discovery of transfusion of blood. But Libavius was the first to give an account of his performance of the operation in 1594. Mr. Lawson Tait tells the story of the introduction of the practice. He says:\*

"This operation was not initiated, as asserted by Mr. Gamgee, in the second half of the seventeenth century by Dr. Lower, of Oxford, nor was it first proposed as a legitimate surgical operation at all. It was proposed, and in all probability was really practised, by the alchemists of the sixteenth century as an attempt to obtain for the wealthy aged a renewal of their lease of life, after the theory and legend of Faustus. Certain it is that allusions to it are frequent, though the first actual account of its performance is given by André Libavius, Professor of Medicine at Halle (Helmst. 1602), as having been performed by him in 1594, the blood of a young healthy man being transfused into a man aged and decrepit, but able and willing to pay for the supposed advantage. In the early part of the seventeenth century, it was a good deal discussed from this point of view, forgotten for a while, and then after the Restoration it was re-considered, and a great deal written about in this country and on the Continent.

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"The scheme of transfusion in all the experiments of the seventeenth century descriptions of which I have seen, was to take arterial blood from an animal and pass it into the veins of another, and that this was successful is not surprising. But this has never been attempted in modern times upon man. It certainly would not be justifiable; because, to interfere with a large artery—and a large artery would be required—in a man is always an extremely risky thing. Dr. Lower, who is Mr. Gamgee's authority, in 1667 injected or tried to inject arterial blood from a lamb into a man, but the operation was so badly done that I do not believe any blood really passed. If Pepys' idea could have been carried out of transferring some of the peaceful blood from the arteries of a member of the Society of Friends, for the replacement of the turbulent and brutal spirit of Archbishop Laud, some good might have been done, much

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\* *Uselessness of Vivisection*, pp. 24—26.

of the terrible history of that time need not have been written, and I might not have appeared here as a critic of such experiments. But no such or any other good result was obtained. A large army of experimenters rushed into the field, a fierce controversy took place; but before the eighteenth century dawned the whole thing was discredited and forgotten. Mr. Flint South gives a succinct history of the matter, and tells us that it was revived by the plan of mediate transfusion in the early part of the present century. The former experiments were fruitlessly repeated and others tried. The result is that the operation has a very insecure hold on professional opinion. I have seen it performed seven times without success in a single instance. I have twice been asked to do it, and have declined, and both patients are now alive and well. We hear a great deal of cases in which patients have survived after transfusion has been performed, but we hear little or nothing of its failures. Personally, I have no confidence in the proceeding."

*Q. Is the operation much practised now?*

A. Very little is heard now of the operation for transfusion of blood, but severe hæmorrhage is often treated by transfusion of saline solutions into the veins of the patient. The circulation is aided in a purely mechanical way says Goltz, and it even excites the circulation.

*Q. Has the treatment of apoplexy been modified in consequence of Harvey's discovery?*

A. Very little. Nor has that of congestion of the lungs. Celsus, the old Roman writer, recommended bleeding in these cases, and the treatment is still followed.

*Q. What about dropsy?*

A. We rarely cure it, but we relieve it by tapping, and Celsus tells us this was practised in his time for the same disease.

*Q. Have not a great number of experiments been performed on animals with respect to what is called the blood pressure which must have an important bearing on the question of the pulse rate?*

A. That is so, but they have been strangely vitiated by the mode of investigation. Thus Dr. A. Morison writing in the *British Medical Journal*, March 14th, 1896, page 650, said:—

"From the days of Stephen Hales (1727) until now, the relations between artery and manometer [a U-shaped tube partially filled with mercury] have not been such as obtain in nature, and the arguments from quadrupeds to man as to the power of the heart and pressure in the blood-vessels are fallacious, for those physical reasons which bring about a difference in the circulatory apparatus of animals habitually and respectively horizontal and erect."

*Q. Is it possible that physiologists have endeavoured to study the question of the blood pressure in man—an upright animal—by experimenting on four-footed or horizontal creatures?*

A. That is so. The Rev. Stephen Hales (1727) was the first to introduce a long glass tube into the femoral artery of a horse in order to estimate the blood pressure by measuring the height of the column of blood. From the height of the column of blood he calculated the force of the heart. Ludwig combined this with a revolving cylinder moved by clock-work, and called a kymograph. When the cylinder is used it is covered with smoked paper, upon which is obtained a blood-pressure tracing.

*Q. Of what practical value to physiological medicine have these experiments been?*

A. Dr. James Macaulay says\* :—

“As to the experiments on the statics and dynamics of the circulation, from those of Hales to those of Ludwig, no doubt many facts have been ascertained and recorded, as is the case with all experiments, but no new or practical results appear ‘for the benefit of humanity.’ As to the absolute force of the heart, considered as a hydraulic machine, and the velocity of the blood, the results of experiments vary much, and those of old Stephen Hales give probably as near an average estimate as can be expected. But for practical application in medicine the numerous experiments made since the time of Hales are quite useless. The force of the heart, for example, varies in the animals inspected, and under different conditions; and the variations are infinite in different persons, in various conditions of age, strength, and state of health. The general estimates may be interesting as facts for philosophical statement, but are useless with any view of applying such experiments to use in maladies either of the sanguineous or nervous system. More useful information can be obtained by observing the force of the heart as indicated in the delicate dial of a balance chair than from all the experiments of vivisectors.”

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\* “Vivisection”: a Prize Essay, p. 41.

## CHAPTER XI.

## THE MOVEMENTS AND SOUNDS OF THE HEART.

*Q. I was recently asked : " How could the action of the valves of the heart or the significance of the sounds of the heart have been discovered save by experiments on the living animal ? "*

A. The action of the valves of the heart has been learned by dissection of the organ, and not by vivisection. The study of its mechanism reveals its functions. The cause of the first sound of the heart is a matter of dispute even now. All that vivisection could teach us and more has been learned on the hearts of frogs and other animals which continue to beat long after the creature's death, and when the organ has been removed from the body. The heart of a frog has been known to beat for two and a-half days after it has been cut out from the animal, a rabbit's heart will beat for many hours under such circumstances, and it is by experiments on these that physiologists have ascertained most of our knowledge of the innervation of the heart.

*Q. Yet I suppose many cruel experiments have been performed in connection with the study of the heart's action ?*

A. Yes. And yet the whole matter is still involved in obscurity. Some physiologists attribute the cause of the first sound of the heart to one thing and some to another.

*Q. To what is the first sound supposed to be due ?*

A. One of the chief factors is certainly due to the " *muscle sound* " produced by the contracting muscular fibres of the ventricles.

*Q. Can this be heard in the dead subject ?*

A. It can. Landois and Stirling say\*: " The sound is heard although enfeebled in an excised heart in which the movements of the valves are arrested. "

*Q. You say that the hearts of some animals continue to beat after they are cut out of the body ?*

A. Yes. Landois and Stirling state† that a rabbit's heart

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\* *Text-Book of Human Physiology*, 3rd Ed. p. 72.

† *Ibid*, p. 74.



beats from 3 minutes up to 36 minutes after removal from the body. Waller and Reid recorded the ventricular contractions of a rabbit's heart 72 minutes after its excision. A mouse's heart will beat under these conditions 46 hours; a dog's 96 hours. An excised frog's heart as I have said has been known to beat for two and a-half days. It is obvious that many experiments on the heart's movements can be performed without any cruelty whatever.

*Q. Have any experiments been performed on the hearts of human beings?*

A. There have been many. The best known were made on Frau Serafin, by Prof. von Ziemssen. The victim had a large part of the front of the chest removed by an operation, and the heart being covered only by a thin membrane, a great number of experiments were performed on that organ.\* Landois and Stirling say that, "in the case of Frau Serafin Von Ziemssen found that the number of beats was doubled when a constant uninterrupted strong current [of electricity] was passed through the ventricles. . . . In Frau Serafin's case the electrodes were applied to the heart, separated from it merely by the pericardium."

*Q. Would not errors be rendered possible by experimenting on the hearts of living animals?*

A. Certainly they would. Dr. Hope says:—

"I have always found that when the animal unfortunately retained or regained the slightest degree of sensibility, the action of the heart was so violent, convulsive, and rapid as to present the alternate action described by Majendie." "Dr. Hope acknowledges his experiments on the heart to have had no other object but that of illustration, for he remarked: 'From experiments on small animals, supported by analogical arguments derived from pathology, I had previously been able to *infer* the nature of the heart's action.'"<sup>†</sup>

*Q. Dr. Hope's experiments were of little practical value I presume?*

A. Yes. The late Mr. Lawson Tait said of them ‡:—

"The experiments were clumsy and barbarous, and Williams exposed their fallacies at the time. What there is of accuracy about them could have been, and have since been, far better proved

\* *A Text-Book of Human Physiology*, pp. 56, 84, 85.

† *Painful Experiments on Living Animals*. Dr. Wall, pp. 257-258.

‡ *The Times*, November 8th, 1892.

by clinical and pathological observation of incompetent and damaged valves."

*Q.* Yet some of your opponents I believe contend that "Dr. Hope between 1829 and 1839 in the only possible way showed by experiments on living animals precisely how the sounds of the heart were produced, and by what changes in the valves or cavity they might be modified in this or that manner"? \*

A. This is of course absurd; as I have just told you the question of the first sound of the heart is not settled even now, yet the writer in *The Times* said experiments have shown precisely how the sounds of the heart are produced. If Hope settled the question before 1839, why do the Text-Books of Physiology say to-day of the first sound, "Yeo and Barrett state that the sound is purely muscular" (?), and append to the statement the note of query, and also tell us that "Wintrich, by means of proper resonators, has analysed the first sound, and distinguished the clear, short, valvular part, from the deep, long, muscular sound"? \*

*Q.* What does Dr. Hope himself say of his researches?

A. On page 28 of his book, "*On Diseases of the Heart*," he says: "Conscious of the gap that was presented in the treatment of diseases of the heart, I have devoted more attention to this than to any other department of the subject, availing myself in particular of the wide and favourable sphere for observation afforded by a long residence as house physician and surgeon in the Royal Infirmary of Edinburgh, where, living literally, I may say, as well as figuratively, at the bedside of the patient, I had an opportunity of closely watching every habitude and phase of the disease, every operation and effect of remedies."

*Q.* Then it was at the bedside and not in the laboratory that Dr. Hope made his discoveries?

A. Exactly. He says (page 76): "Mitral valves.—(1.) Systolic murmur, that is from regurgitation. It was the existence of this murmur in Christian Anderson (a patient in the infirmary) who had no disease of the semilunar valves, that led me to the detection of regurgitation in general, in June, 1825."

*Q.* Did Dr. Hope put much faith in researches on living animals?

A. Evidently not, for he says, referring to the errors of

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\* *The Times*, October 25th, 1892.

Majendie: "It is easy to see how Mr. Majendie has been misled, namely, by operating upon *living* animals."

*Q. I suppose that experiments on the movements of the heart are still being made?*

A. Yes. For example, Dr. Berry Haycraft and Dr. D. N. Patterson contributed a paper to the *Journal of Physiology*, vol. xix, p. 496, wherein they say:—

"In the dog with an opened chest we can learn by inspection that the heart decreases in all dimensions during systole, provided we are careful to inflate the lungs so as to buoy up the heart into its natural position."

The experimenters thrust needles into the left side of the chest of a rabbit, and attached "light straws" to the heads of the needles projecting outside the chest, so as to enable them to study the position of the heart "during the cardiac cycle." If Dr. Hope's experiments settled everything about the movements of the heart before 1839, why are animals being cruelly tortured with such experiments to-day?

## CHAPTER XII.

### RESPIRATION.

Q. *Have experiments been made upon animals in connection with the study of the respiratory movements?*

A. Many, and some of them exceedingly cruel ones, and for the most part useless, too.

Q. *Give me an example.*

A. We are told that

"If the contents of the abdomen are removed from a living animal, every time the diaphragm contracts the ribs are drawn inwards. This, of course, hinders the chest from becoming wider below, hence the presence of the abdominal viscera seems to be necessary for the normal activity of the diaphragm."<sup>\*</sup>

Q. *This demonstrates the use of having bowels. I confess it was hardly necessary.*

A. Even in the case of the respiration we remark the difference in the physiology of various animals. For example, in some experiments on the chest muscles, by Martin and Hartwell, it was discovered that

"The internal intercostal muscles are expiratory throughout their whole extent, at least in the dog and cat; and that in the former animal they are almost 'ordinary' muscles of respiration, while in the latter they are 'extraordinary' respiratory muscles."<sup>†</sup>

Rutherford thought that the internal intercostals are probably muscles of inspiration. Even these experimenters could not definitely settle the point.

Q. *I have heard that so many things influence the respiration that doctors who have to count the number of times a patient breathes per minute are compelled to do so unobserved by the patient*

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<sup>\*</sup> *A Text-book of Physiology.* Landois and Stirling, 3rd edition, p. 173.

<sup>†</sup> *Ibid.*, p. 175.

*as attention to the act of breathing exerts a marked influence upon the function. Is that so?*

A. It is, and of course introduces numberless fallacies in experiments on the subject. Dr. Barclay, who wrote *On the Muscular Motions*, said (p. 298):

"In making experiments on live animals, even when the species of respiration is the same as our own, anatomists must often witness phenomena that can be phenomena only of rare occurrence. After considering that the actions of the diaphragm, in ordinary cases, are different from its actions in sneezing and coughing, these again different from its actions in laughing and hiccough; after considering that our breathing is varied by heat and cold, by pleasure and pain, by every strong mental emotion, by the different states of health and disease, by different attitudes and different exertions,—we can hardly suppose that an animal under the influence of horror, placed in a forced and unnatural attitude; its viscera exposed to the stimulus of air; its blood flowing out; many of its muscles divided by the knife; and its nervous system driven to violent desultory action from excruciating pain, would exhibit the phenomena of ordinary respiration. In that situation its muscles must produce many effects, not only of violent, but irregular action; and not only the muscles usually employed in performing the function, but also the muscles that occasionally are required to act as auxiliaries. If different anatomists, after seeing different species of animals or different individuals of the same species respiring under different experiments of torture, were each to conclude that the phenomena produced in these cases were analogous to those of ordinary respiration, their differences of opinion as to the motions of ordinary respiration would be immense."

Q. *Have not many experiments on animals been performed on the subject of Suspended Animation?*

A. Yes, and a full account of them may be found in the *Report of the Royal Commission on Vivisection*, pp. 365-67. Seventy-six experiments were made on animals, in only a few of which anæsthesia was present; they were conducted by a committee appointed by the Royal Medical and Chirurgical Society. The Royal Humane Society suggested the investigation, but was in no way answerable for the cruel proceedings of the medical body undertaking the investigation.

"After terrible suffering," says the *Report of the Royal Commission*, "caused by plugging their windpipes [*i.e.*, of the animals used in the research] to suffocate them, holding them under water, and in some cases restoring them to life for further experimentation, burying their heads in liquid plaster of Paris, or mercury, cauterizing their bodies with an iron heated to a white heat, etc., etc., the committee, it will be seen, report that they were unable to recommend any material improvement in the plan adopted by the Society."

Q. *Is it the fact that experiments in suffocating animals are sometimes performed in the presence of pupils at ordinary schools and colleges?*

A. A Memorandum was issued a few years ago in Girton College, Cambridge, from which it appears that the students attended demonstrations on living animals in connection with the movements of the diaphragm, apnoea, dyspnoea, and the action of the respiratory muscles in dyspnoea.

Q. *What is meant by apnoea?*

A. Privation of breathing.

Q. *And dyspnoea?*

A. That is difficult breathing.

Q. *What took place before the students?*

A. In this experiment (Lesson XVIII.) a rabbit was first given chloral, which is not an anæsthetic, but a stupefier. Then the animal was opened so as to show the muscles of the chest and abdomen at work, and the effects of obstruction by normal breathing and of suffocation. The abdomen was opened to show the action of the diaphragm, which is the movable muscular partition between the chest and belly.

Q. *I presume that all this is quite ordinary procedure in physiological laboratories?*

A. Certainly it is, and the text-books of physiology prove it. Thus Landois and Stirling say:—\*

“The phenomena of asphyxia may be developed by closing the trachea of an animal with a clamp or by any means which prevents the entrance of air, or blood into the lungs.”

Again, they say: †

“We may measure the blood-pressure in any artery of an animal while it is being asphyxiated, or we may open its chest, maintain artificial respiration, and place a manometer (a U-shaped tube partially filled with mercury) in a systemic artery, *e.g.*, the carotid and another in a branch of the pulmonary artery. In the latter case, we can watch the order of events in the heart itself, when the artificial respiration is interrupted. It is well to study the events in both cases.”

Q. *Tell me what is meant by artificial respiration in this connection?*

A. For physiological purposes artificial respiration is often resorted to, especially after the use of curare. Air is forced

\* *A Text-book of Human Physiology.* 3rd edition, p. 196.

† *Ibid.*, p. 197.



into the lungs by means of bellows attached to a tube tied in the windpipe, the bellows is worked by a machine.

*Q. What is the action of curare?*

A. Curare, the Indian arrow-poison of South America, when injected under the skin or into the blood of an animal, acts chiefly on the motor nerve-endings, but does not affect sensation. When completely under the influence of the poison the animal "lies in any position, limp and motionless, neither exhibiting voluntary nor reflex movements."\*

*Q. Can the animal feel pain?*

A. The most distinguished physiologists are agreed that a creature under the influence of this drug feels pain, and we believe even more acutely, because it has no means of expressing its agony.

*Q. Cannot the animal utter cries?*

A. Outwardly it is a corpse, inwardly a sentient creature. It is for this influence and the animal organism that Lord Tennyson spoke of the drug as "the hellish woorali."

*Q. What do physiologists say of its action?*

A. Claude Bernard, who first investigated its properties, said:

"Curare acting on the nervous system only suppresses the action of the motor nerves, leaving sensation intact. Curare is not an anæsthetic. . . . Curare renders all movement impossible, but it does not hinder the animal from suffering and from being conscious of pain."†

*Q. Is that opinion confirmed by other physiologists?*

A. Certainly it is. Professor Holmgren said:

"Under its influence no creature can give the faintest indication of its hopeless condition. . . . this venom is therefore the most cruel of all poisons."‡

*Q. Have you a more recent expression of opinion from an authoritative source, because I have heard it stated that curare is an anæsthetic in large doses?*

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\* *A Text-book of Human Physiology*. 3rd edition, p. 471.

† *Revue Scientifique*, 1871-2, p. 892; also Vol. vi., p. 591.

‡ Holmgren's "*Physiology of Present Times*," 1868, p. 231.

A. In an important book, entitled "*Semi-Centennial of Anæsthesia*,"\* p. 67, the author says :

"Curare is a drug which has important uses in a certain class of experiments upon animals. It has never been claimed by any scientific man that it is an anæsthetic."

Q. *Does curare paralyze the heart ?*

A. No. The heart alone continues to beat, but without artificial respiration the animal would die, because the drug paralyzes the muscles of respiration.

Q. *Curare must be very useful for keeping the animal quiet under experiment ?*

A. This is one of its most important uses in the laboratory, as I shall explain at our next meeting.

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\* By Dr. W. H. Welch. Boston, 1897.



## CHAPTER XIII.

### CURARE.

Q. *What is Curare and why is it used in the laboratories?*

A. Curare is a drug obtained from certain species of strychnos. It is variously called Woorari, Wourali, Urari and Curara or Curare. It paralyses the peripheral ends of motor nerves even when given in very minute doses. Large doses paralyse the vagus nerve and the ends of sensory nerves.\*

"It causes general paralysis of all striped muscular tissue, excepting that of the heart, by an action upon the motor nerve endings. It kills by paralysing the muscles of respiration."†

Q. *It is used I presume to paralyse animals under the vivisector's knife?*

A. Yes. Claude Bernard, who made the first important physiological researches on this drug, said:—

"Curare is now employed in a vast number of experiments as a means of restraining the animals. There are but few observations of which the narrative does not commence by notifying that they were made on a curarised dog."‡

Q. *But if it kills by paralysing the muscles of respiration how can the animal under its influence be kept alive?*

A. It is preserved from death by artificial respiration. That is to say, the animal's throat is cut, a tube is inserted in the windpipe and a machine is used to force air into the animal's lungs during the whole time the effect of the drug remains.

Q. *Is it an anæsthetic?*

A. It is not.

"Curare," says Claude Bernard, "acting on the nervous system only suppresses the action of the motor nerves, leaving sensation

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\* *Pharmacology, Therapeutics, and Materia Medica.* T. Lauder Brunton, 1885, p. 889.

† *Handbook of Therapeutics.* Ringer & Sainsbury. 13th Ed., p. 502.

‡ *Physiologie Opératoire* (Paris, 1879, p. 168).

intact. Curare is not an anæsthetic. . . . Curare renders all movement impossible, but it does not hinder the animal from suffering and from being conscious of pain.”\*

Q. *What is the appearance of an animal under the influence of curare?*

A. It appears like a corpse, but the appearances are most deceitful. Says Claude Bernard :—

“In this motionless body, behind that glazing eye, and with all the appearance of death, sensitiveness and intelligence persist in their entirety. The corpse before us hears and distinguishes all that is done around it. It suffers when pinched or irritated; in a word, it has still consciousness and volition, but it has lost the instruments which serve to manifest them.”†

Another professor of physiology, F. Holmgren, of Upsala University, says :—

“There is a poison (curare) which lames every spontaneous movement, leaving all other functions untouched. This venom is therefore the most cruel of all poisons. It changes us instantly into a living corpse, which hears and sees and knows everything, but is unable to move a single muscle, and under its influence no creature can give the faintest indication of its hopeless condition. The heart alone continues to beat.”‡

Q. *Did not you say that large doses of curare paralyse the ends of the sensory nerves, and if that is the case, why is it not an anæsthetic?*

A. The “large doses” which I mentioned explain your difficulty. Professor Schiff, a notorious vivisector at Geneva, said :—

“It is nothing but hypocrisy to wish to impose on oneself or on others the belief that the curarised animal does not always feel pain.”§

\* *Revue Scientifique*, 1871-72, p. 892; also Vol. vi., p. 591.

† *Revue des deux Mondes*, Vol. 53. Second series (September 1st, 1864), pp. 173 and 182.

‡ Holmgren's “*Physiology of Present Times*,” 1868, p. 231.

§ *Sopra il Metodo seguito negli esperimenti sugli animali Viventi*, &c., p. 37. (Firenze, Andrea Bellini.) In a second edition of this pamphlet, Professor Schiff says (pp. 34-36) :—

“Curare given in *great doses* really abolishes sensibility, but . . . it only acts as an anæsthetic a long time after having destroyed voluntary movements and automatic respiration.”||

|| See further on this question in *Anti-Vivisection Evidences*. By B. Bryan. (London, 1895.) P. 94.

*Q. The purpose for which curare is used is, I presume, to keep the animal quiet and so make the experiment an easier one to conduct?*

A. Yes the creature can neither bite, scratch, cry out, nor struggle. The victim remains plastic as clay under the torturer's instrument.

*Q. Is that the reason why Lord Tennyson called the drug "the hellish wourali"?*

A. Yes. Dr. W. H. Welch says:—

"In a limited class of cases its employment, either with or without the coincident addition of anæsthetics, is indispensable."\*

*Q. Is curare used then in conjunction with chloroform?*

A. Dr. Lauder Brunton was asked this question in the course of his examination before the Royal Commission.

"Is there anything to prevent your giving both drugs, or giving them mixed together, so as to stop the pain by the chloroform and the nervous movement by wourali [curare]?" "Yes, there is, and it is this; in very many of these experiments you want to ascertain what is termed the reflex action; that is to say, that an impression is made upon a nerve, and goes up to the cord, and is transmitted down. Now, chloroform acts upon the reflex centres, and abolishes their influence completely; so that if you give the wourali [curare], which paralyses the ends of the motor nerves, and give the chloroform, which paralyses the reflex centres, you deprive yourself of the possibility, in many instances, of making satisfactory experiments."

"But are there not many instances in which you give wourali simply for the purpose of getting the animal perfectly quiet?" "Yes, those instances which I gave." "But if it is done for the purpose of getting the animal perfectly quiet, could not chloroform be given also?" "No, for this very reason; if you were to give chloroform the experiment would be at an end; you would have abolished the action of the reflex centres, and then you might as well not do the experiment at all."†

*Q. I have been thinking over what you said about curare not being an anæsthetic but only a paralyzer of motion, and I should*

\* *Semi-Centennial of Anæsthesia*. By Dr. W. H. Welch. (Boston, 1897.) P. 67.

† *Evidence Royal Commission*, London, 1876. Q. 5,743 (5,745). Dr. Brunton afterwards sought unsuccessfully to modify the effects of his evidence. The contradictions he gave are exposed in "The Modern Rack" (London, 1889, pp. 142, 143).

*be glad if you could give me some proof that an animal under its influence is really conscious ?*

A. Your question is an exceedingly difficult one to answer. To bring an animal under the full influence of curare as a paralyzing agent, the muscles of respiration must be put out of action; this necessitates the performance of artificial respiration, the animal's throat must be cut and a tube inserted in the windpipe to enable air to be forced into the lungs, as I have explained already. It is obvious that this experiment could not be performed legally on a human being, so as to enable him to describe his sensations after recovery. We cannot obtain the information from an animal, and therefore there is no satisfactory reply to be made to your query; but I will give you all the information that is to be had upon the matter. The late Dr. Henry Lionville, Chief Director of the Laboratory, Hotel Dieu, Paris, gives a description of a case where an over-dose of curare had been given to a patient and artificial respiration had to be kept up till the patient recovered. He says:—

“The patient then related all he had felt, the preservation of the intellect, the annihilation of all power of movement, of which he gave a clear account, witnessing all that went on around him, without being able to take any part in it, the fears freely expressed by some young assistants present being by no means reassuring to him.”\*

Q. *Surely there must have been other accidents if not actual experiments of the same character ?*

A. Claude Bernard had something to say on this. He tells us †:—

“We have the accounts of individuals who have been inoculated with curare, but to a degree which had not stopped the motions of respiration, and consequently permitted the individuals to return to movement. These have been able to relate that during their paralysis they had nevertheless been fully conscious of their existence and of all the impressions on their senses.”

Q. *Did not Professor Gamgee before the Royal Commission state that he had himself performed some experiments with curare on children ?*

A. Yes. And he said that in consequence

“I was able to determine, very decidedly, that sensibility was not at all impaired; although there was a certain amount of paralysis

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\* *Bulletin Général Thérapeutique*, November, 1865, p. 404.

† *Revue Scientifique*, 1874, p. 1117.

of motion produced by the curare, there was no affection of the sensory nerves. And this observation I was able to confirm by others on another little patient."\*

Q. *Where were these experiments performed?*

A. At the Children's Hospital at Manchester.

Q. *Why were the little patients subjected to such a research?*

A. Professor Gamgee said that it had been suggested by a German physician that small doses of curare were useful in cases of epilepsy. In consequence of this he tried the drug in a very careful manner on almost hopelessly epileptic children. One little girl, after a minute dose, could not raise her arms, and was unable to walk, yet he was able to determine very decidedly that the child's sensation remained intact.

Q. *Is there any evidence tending to show that curare is an anæsthetic when administered to human beings?*

A. A case was reported in the *British Medical Journal* of March 16th, 1889 (p. 598), by Mr. Joseph White, a Nottingham surgeon. The case was that of a servant girl accidentally wounded by an Indian arrow, by which she was poisoned. She became unconscious and in that condition the operation of cutting out the poison was performed. When she recovered she declared that she knew and had felt nothing of it.

Q. *That seems to prove that curare does exert some anæsthetic influence?*

A. Not at all. There is no proof that the arrow was poisoned by curare. Savages poison their weapons with other deadly compounds, and Mr. White only said "he believed that the arrow had been poisoned with curare." In face of the authentic cases I have already cited and the fact that the Royal Commission clearly laid it down that curare is not an anæsthetic, we cannot be expected to attach much importance to Mr. White's mere belief.

Q. *If I understand you correctly curare is of great assistance to the experimental physiologist?*

A. Yes, I will give you an illustration of this. The late Professor Rutherford performed certain experiments on dogs

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\* *Minutes of Royal Commission on Vivisection*, Q. 5,407.



and reported his results in the *British Medical Journal*. The dogs were starved for many hours. They were then fastened down; the abdomen was cut open and the bile duct was dissected out and cut; then a glass tube was tied into the bile duct and brought outside the body. The duct leading to the gall-bladder was then clamped and various drugs were put into the intestines. Dr. Rutherford in his *Syllabus of Lectures on Physiology* (Part I, 1890, p. 70) thus describes the manner in which he used curare on the victims of his very cruel research. He says:—

"The disturbing effect of irregular muscular movements upon the biliary flow was prevented by injecting into a vein small doses of curara, repeated at intervals when the motor paralysis which it induces became too slight. In consequence of the curara palsy, artificial respiration was had recourse to, and maintained at regular intervals throughout the whole experiment.

"Chloroform was used during the preliminary operation in two cases, but the stimulation of the liver which it induced rendered the experiments worthless. On the other hand, we have abundantly proved that the doses of curara administered in the experiments have no influence on the biliary secretion, and do not interfere with the effects of hepatic stimulants. It is therefore an exceedingly valuable auxiliary in a research of this nature."

Q. In a book I have just been reading, entitled "*Experiments on Animals*," by Mr. Stephen Paget, it is maintained, p. 243, that curare "in such doses, as are given in those few cases where it is used," acts not only on the motor system, but also on the sensory system. What do you say to this?

A. As curare is not an anæsthetic under the Act, and as the vivisectors declare that it is not used alone, but combined with some pain-annulling drug, I cannot see the advantage of using curare in such doses as would affect the sensory system, presuming it can exert that influence. Curare, it is useless to deny, is employed in the laboratories to keep the animal motionless.

Q. Mr. Paget certainly makes a very feeble attempt to prove the anæsthetic part supposed to be played by the drug.

A. Yes. He gives us the *ipse dixit* of Prof. Ruffer, and quotes from the *Edinburgh Review*\* the case I mentioned to you, which was reported by Mr. White, the Nottingham surgeon,† where the servant was accidentally wounded

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\* July, 1899, article on Vivisection.

† See this *Catechism*, p. 66.

by a poisoned Indian arrow. As I said there is no proof that the arrow was poisoned by curare, and the girl may have become unconscious from other causes than that of the anæsthetic effect of anything inoculated by the weapon.

Q. *Is Dr. Ruffer correct in saying that "curare is nowadays not used alone, but is always used in combination with morphia, ether, chloroform, or other anæsthetics"?*

A. This is not the fact. I have already quoted a passage which proves the contrary from Professor Rutherford's Syllabus of Lectures on Physiology.\* In the July number of the *Journal of Physiology* for 1893, there is an article by Dr. G. N. Stewart, of Cambridge, on the circulation of the blood, which contains the records of a great number of experiments on dogs and rabbits. The paper is a very lengthy one, and the experimenter states precisely what he administered to each of the animals, in the way of drugs, before the experiment began; in one case he used chloral and curare, in three cases morphia and curare, in one case atropine and curare, but in ten cases he used curare alone.

Q. *Of what character were the experiments in which curare alone was used?*

A. Curare was used alone in the case of a bitch, concerning which it is recorded, p. 80:

"Artificial respiration—exposed right internal jugular vein, right carotid artery and right vago-sympathetic nerve below thyroid gland. Carefully separated right vagus from sympathetic for some distance. Cannula in left external jugular vein."

Electricity was used for stimulating the cut nerves, the operation began at 2.2 and ended at 8.14, having thus lasted 6 hours and 12 minutes, under curare alone. In the case of a dog, the experiments on which are recorded on pp. 86-87, we read:

"Put under morphia for operation. Cannula in left external jugular. Exposed right internal jugular and right carotid below thyroid; 20 mg. curare into blood, as animal could not be got properly under morphia. Artificial respiration."

This experiment began at 2.42 and ended at 7.35, thus lasting under curare close upon five hours.

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\* See this *Catechism*, p. 67.

*Q. Can you give more proof that curare is used merely to restrain the animal?*

A. Certainly I can. In the *Journal of Physiology* for September, 1893, there is a paper "On the Results of Ligation of the Coronary Arteries," by Mr. W. Townsend Porter. The author says (p. 127):

"Dogs were used in my experiments. The second, third, fourth and fifth dog of the series of thirty-two recorded here were given a small quantity of morphia. Voluntary movements were prevented by curare. The heart was reached by a nearly bloodless re-section of part of the six upper ribs on the left side. The edges of the pericardium were stitched to the edges of the thoracic opening."

The time occupied by these awful vivisections varied from 18 to 100 minutes. In the course of the investigation, fifty arteries were prepared for tying.

*Q. I understand that curare is not a pure substance?*

A. No. Dr. Lauder Brunton says\* :—

"Many of you may know that the curara prepared by the inhabitants of the Amazons and Orinoco is not a pure substance, but that it consists chiefly of an extract from a particular species of strychnos, which is the active part, and that this is mixed with a mucilaginous substance to make it sufficiently sticky to adhere to the arrows. In addition to this, however, some tribes mix with it the venom of certain serpents."

*Q. Does not this explain the peculiarities of the case of the girl who was supposed to have been anæsthetised by the action of curare?*

A. It throws light on the abnormal action if indeed there were any curare on the arrow at all.

*Q. What does Dr. Lauder Brunton, who is himself a vivisector, say about the physiological action of curare?*

A. He says curare paralyses the ends of the motor nerves and thinks that the poison may paralyse some other structures than the motor nerve endings, and quotes Schiff's experiments as proving that when the poison has acted for a considerable time it does paralyse the ends of the sensory nerves as well as of the motor.†

*Q. I want to understand this matter thoroughly. How could an experimenter tell when he had given the animal not only*

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\* *Action of Medicines*, p. 145 (1897).

† *Ibid.*, p. 149.



*sufficient curare to paralyse the motor nerves, but also to paralyse the sensory nerves as well?*

A. It is practically impossible for him to do anything of the kind. "We cannot tell" from the experiments, says Dr. Brunton,\* "what action it has or has not upon the nerve centres, because having paralysed the ends of the motor nerves, we have taken away the only index by which the nerve centres can give evidence of their functional activity."

Q. *I concluded as much. It seems then that it is not quite honest of the vivisectors to insist as some do that curare as they use it is in any sense an anæsthetic?*

A. Just so. The drug is never used as an anæsthetic except when it is necessary to anæsthetise the public conscience.

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\* *Action of Medicines*, by Dr. Lauder Brunton, p. 147.

## CHAPTER XIV.

### ANÆSTHETICS.

Q. *What is an anæsthetic?*

A. Before the Anti-vivisection controversy became so acute there was no difficulty in defining this term at all. Thus Webster's Dictionary, 1864, explained it as follows: *Anæsthesia*—A state of insensibility produced by the inhalation of chloroform or other agents. *Anæsthetic*—Capable of rendering insensible by being inhaled; that which produces insensibility, as chloroform, &c. *Anæsthetize*—To render insensible by means of an anæsthetic, as chloroform, sulphuric ether, &c.

Q. *Surely this is what every surgeon means by these terms. Do you mean to say that the words are now used in a different sense?*

A. Yes. For their own purposes the vivisectioners have not scrupled to amend the dictionaries. They now wish us to believe that drugs such as chloral and morphia are anæsthetics, though, of course, as everybody is aware, they are merely stupefiers.

Q. *But if I go to a surgeon for an operation and he promises to give me an anæsthetic while he performs it, surely he means to give me what I expect, chloroform or ether at his discretion.*

A. Certainly, but vivisection is not in the least like surgery.

Q. *Yet it is often declared that the experimenters treat their victims exactly as surgeons treat their patients in their operations.*

A. I have seen this statement repeated many times. It is obvious to any reflecting mind that there is all the difference in the world between a surgical operation with the object of a cure and that of a vivisection for the purpose of experiment. The surgeon wishes his patients to get well, the experimenter desires his victim to afford him the knowledge he seeks.

*Q. I presume that some experiments could not be performed under anæsthetic conditions ?*

*A.* In many cases the anæsthetic (1) interferes with the result of the experiment ; (2) is dangerous to the life of the animal if given effectively ; or (3) cannot be maintained for more than a small portion of the time in which the pain continues.

*Q. Give me an example of the first class ?*

*A.* Professor Rutherford told the Royal Commission :

"One did not dare give the chloroform because of the risk of having the results possibly interfered with. I advisedly in this research never gave chloroform."\*

Again, Dr. McDonnell told the same body :

"I have repeated at the time that they were quite new and *sub judice*, the researches of Dr. Brown-Séquard upon the spinal cord of animals. In those cases I believe it is unavoidable to have suffering."†

Once more, Professor Sharpey said with reference to the facts about Sir Charles Bell's Experiments on the Nerves :

"They could not be used under anæsthetics. You see anæsthetics would destroy the sensibility, and you could not use such an experiment under anæsthetics. And it is a painful experiment in two ways : in the first place, exposing the spinal cord and laying bare the roots of the nerve is a very painful operation ; and that part of it might be done under anæsthetics, and then the animal may recover its sensibility ; but the subsequent prolongation of the experiment would be painful."‡

*Q. About your second class—"dangerous to the life of the animals." Can you illustrate that ?*

*A.* In the great work of the late Mr. T. R. Lewis, M.B., F.R.S., Assistant Professor of Pathology in the Army Medical School, the experimenter laments the fact that chloroform is so very fatal to rats and rabbits, as also to puppies and young dogs, and he says : "Even in large healthy dogs we calculate on losing one in five through this cause alone." Mr. E. W. Hoare, Veterinary Surgeon, says :§

"In many instances respiration ceases almost without any preliminary warning, and in some cases death occurs with such rapidity that it would appear as if syncope was the cause."

\* *Report of Royal Commission*, Q. 2,932.

† *Ibid.*, Q. 4,487-8.

‡ *Ibid.*, Q. 464.

§ *Veterinary Journal*, Vol. XXXVII, No. 219, p. 163.

Mr. William Pritchard, late Professor Royal Veterinary College, told the Royal Commission :

"With regard to dogs, I should never think of applying chloroform at all; I should think it very unsafe to do so. The dog has an intermittent pulsation; the heart's action is intermittent." \*

Q. *What about the difficulty of maintaining the anæsthesia while the pain lasts?*

A. I have already told you that Professor Sharpey explained with reference to experiments on the functions of the nerves that although the initial operation for exposing the spinal cord could be done under anæsthetics, the actual experiment on the nerves must be done when the animal recovers its sensibility. Experiments on nerves of sensation obviously require sensibility in their subjects. Dr. McDonnell told the Royal Commission† that "anæsthetics cannot however be used in experiments with regard to the nerves of sensation."

Q. *I take it that some experiments cannot be really said to begin till the preliminary operation of exposing, say, the heart, kidneys or intestines is completed.*

A. Exactly, and in these cases the anæsthetic is often stopped when the initial operation is completed and what is termed the period of observation begins.

Q. *I should like to hear this explained more fully?*

A. In the *Journal of Physiology*, Vol. XXIII., No. 6, 1899, is an article by Dr. Rose Bradford on the Kidney, which details the results of the removal of large portions of the kidneys from forty-nine living dogs. The object of these experiments was to discover how the animals could sustain life, and for how long, with portions of their kidneys cut out from time to time. The effects of the anæsthetics having passed off, the *period of observation* began when anæsthetics of course, could not be employed. One dog died from loss of blood on the sixth day, another on the fourth; one died thirty-six days after the operation. Great emaciation, thirst, weakness, vomiting, loss of appetite, diarrhœa, ulceration, etc., are noted as preceding the deaths. The vivisector could say with truth that his *operation* was performed under anæsthetics, but we know that the anæsthesia was not maintained for six, much less for thirty-six days.

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\* Report, Q. 796-803.

† Report, Q. 4-486-7-8.



Q. Can you give me further proofs that anæsthetics are not admissible in what you say is the period of observation?

A. In vol. xxi. of the *Journal of Physiology* in an appendix there is an account of some experiments performed by Messrs. Leonard Hill and Harold Barnard. In the preliminary report the experimenters say of their victims, "animals—dogs and cats; anæsthetic, morphia." When their final report was made (p. 323, *et seq.*) the words used are as follows:—

"Animals investigated, dogs—in a few instances, cats and rabbits—anæsthetic employed, chloroform and ether during all operative procedures.

"Morphia narcosis during the periods of recording observations.

"It is absolutely essential that chloroform should not be administered during the periods of observation."

Q. What was the nature of their experiments?

A. The animals had their carotid arteries dissected out and tubes thrust down them. Their abdomens were cut open and the viscera exposed, their splanchnic nerves were divided, and a knife was suddenly passed through the diaphragm so as to open the thorax.\*

Q. If I understand you rightly the animals had an efficient anæsthetic such as chloroform while the cutting operations were performed; is that so?

A. Yes. Then you notice that the chloroform was not to be administered after the actual incisions were made, and so the animals, lying bound to the torture trough, remained with their throats cut, their bowels exposed, and their nerves divided, with nothing to assuage their agony but a small dose of morphia.

Q. Why, "a small dose"?

A. Because the action of morphia on dogs for example is peculiar; thus the *British Medical Journal* (January 14th, 1899, p. 94) says, "this substance acts upon dogs as a violent stimulant rather than as a narcotic, large doses causing excitement and convulsions." In the *Journal of Physiology*, vol. xxiv., No. 2, p. 137, Messrs. Bayliss and Starling say morphia "seems to have a somewhat different effect in the dog since a well-known result of its injection into this animal is the production of vomiting and evacuation of fæces." An overdose of morphia administered to the dog has caused the pulsation

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\* See *The Torture of Animals for the Sake of Knowledge*, by the Hon. Stephen Coleridge, pp. 7-9.

of the heart to become so weak and irregular that its further use appeared dangerous.\*

*Q. Is the operation for preparing blood-vessels for the insertion of tubes a very painful one?*

A. Professor Imanuel Munk injected soap into certain veins of dogs. We are told that in one case, although mention was made of morphia, ether and chloroform, the laying bare of the vein for injection caused the dog to react vigorously, partly by struggling and partly by barking. When the last dose of soap had been injected the dog gave a piercing howl and then was still.†

*Q. I understand now that when we are told that although a vivisection has been performed under complete anæsthesia we are to reflect that it is highly probable the animal has suffered extreme torture when the experimenter has laid down his knives and forceps.*

A. Yes, for the period of observation, the watching the effects of what he has done, must in many cases involve the extremity of agony.

In the *British Medical Journal* of 18th June, 1898, p. 1538, Dr. Cecil E. Shaw of Belfast describes some experiments which he performed in the course of a research on "Sympathetic Ophthalmia." He says:—

"My aim was to discover whether prolonged irritation, external or internal, of a rabbit's eye would produce any visible organic changes in the ciliary region (region of the eyelashes), either in the irritated eye or in the other.

"In the first experiments the eye was cocaineised, and a wound was made in the ciliary region without antiseptic precautions, and the cornea irritated with powdered jequirity seeds for varying periods up to six months. The jequirity caused purulent conjunctivitis (inflammation under the eyelids, attended with discharge of matter) and abundant cocci were present in the pus, but though in some cases the ciliary region was again wounded while the eye was bathed in pus no iritis (inflammation of the iris of the eye) was caused, &c., &c.

"Internal irritation was then tried, small shot being inserted in the eye, again without antiseptic precautions. In fact, soiled instruments were purposely used to reproduce as far as possible the kind of wound which is most often found to cause sympathetic ophthalmia. . . . When the wound was made in the ciliary region more external irritation was seen. In one case the wound opened after several days, the shot was expelled, and all signs of irritation disappeared. In another the shot was similarly expelled, but after

\* *Du Bois Reymond's Archives Supplement*, 1890, p. 116.

† *Ibid.*

some weeks, and irritation is still present at the end of five months. . . . This rabbit I have kept alive to observe how long the irritation lasts, and whether it will yet lead to any changes in the iris or ciliary body."

*Q. I see now that we must be on our guard when we are assured that anesthetics are always used in vivisection and take care to inform ourselves as to the after consequences involved.*

A. Yes, this is the key of the whole position, and we shall have to learn a great deal more about "after consequences" in experiments on animals in cases that involve no actual operation at all before we comprehend the vivisection question.

*Q. I have often heard the objection that the term vivisection is improperly applied to experiments which do not involve the dissection of living animals, a writer in the British Medical Journal recently said "the word" [vivisection] "is blessed and very precious, and an omnipotent weapon in disingenuous hands."*

A. The answer is simple. The word "vivisection" is generally understood (and conveniently employed) to mean "The Practice of Subjecting Live Animals to Experiments for Scientific Purposes." The Royal Commission on the subject used in its report both the full title I have just given and also the short title, "The Royal Commission on Vivisection." It is only for convenience and not for any disingenuous purpose that we use the word vivisection to describe any painful experiment on an animal for scientific purposes.

*Q. Are there many painful experiments which are not "cutting operations"?*

A. A great many. For example the whole class of toxicological experiments or those concerned with poisons.

*Q. Tell me what these involve?*

A. Let us for example take the drug Veratria or Veratrine. Even the minutest quantity of this alkaloid brought in contact with the nostrils causes great and continued irritation. When this is injected under the skin it causes the most intense pain, as though one were burned with hot needles.

Many experimenters have administered it to hogs, horses and other quadrupeds and they tell us it causes colic, violent vomiting and purging, convulsions, tetanus and death. It has been taken as a poison by human beings with painful effects, which in one case did not cease for two months.\*

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\* *The National Dispensatory*, by Stillé and Maisch, p. 1512.



Rabbits poisoned with aconite suffer the most irritating and burning effects and have been observed "to jump vertically in a very peculiar manner, and often to squeal pitiously," then to fall into "severe convulsions." Such torments are not inferior to those caused by the vivisector's knife.

*Q. Are experiments of this kind frequently performed?*

A. Next to bacteriological experiments they are more common than any other.

*Q. Why is this?*

A. Every day some new drug or chemical, discovered for the most part in Germany, is vaunted by the manufacturers as a sovereign remedy for one or other of the diseases common to our species. All these laboratory products, in accordance with the present physiological practice, are tested upon animals. I have just received a manufacturing chemist's list of new pharmaceutical preparations, in which we are told that, "We devote two rooms, each 150 ft. by 30 ft., entirely to the keeping of guinea-pigs, rabbits, rats, mice, &c."

*Q. Are manufacturing chemists also physiologists?*

A. They employ physiologists as *assayers* in pharmaceutical research. Where chemical tests are futile the animal test is employed and this is termed the basis of the New Pharmacy.\*

*Q. Explain what you mean by "the animal test" in this connection?*

A. Manufacturers are employing large numbers of animals whereon to test the strength of their drugs. The physiological standardisation of drugs is based upon the idea of administering to animals doses proportionate to body weight, thus the poisonous power of Ergot is determined upon pregnant animals, if the drug is pushed mortification takes place. In *Therapeutic Notes* for January, 1900, a firm of druggists explain that their "animal outfit comprises a number of sheep and heifers, donkeys, mules, goats, ponies, and 3,000 guinea-pigs, and (for pharmacological work) 500 dozen frogs, besides an abundance of rabbits, pigeons, dogs, roosters, calves, white mice, and white rats—a veritable menagerie."

*Q. Are all such testing experiments painful?*

A. Not all involve equal suffering, but take the following as a fair sample of the distress inseparable from such a method:—

"Thus in testing *Cannabis Indica* we administer it in the form of extract to dogs, with the result that after the lapse of the normal

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\* See the *Bulletin of Pharmacy*, October, 1898.



interval of time the action of the drug becomes manifest in the staggering gait, loss of muscular control, reduction of temperature, etc., followed by insensibility. . . .

"Our pharmacological laboratory is thoroughly equipped for the investigation of the physiologic action of all kinds of potent medicinal agents upon the heart, respiration, blood-vessels, kidneys, intestines, nerve centres and nerve endings, muscles, &c., &c." \*

*Q. Of course anæsthetics could not be used in any toxicological experiments like these ?*

A. They would be out of the question, and yet these experiments are properly considered as vivisections.

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\* *Therapeutic Notes*, January, 1900.

## CHAPTER XV.

### BACTERIOLOGICAL EXPERIMENTS.

*Q. I am told that bacteriological experiments, or those which involve inoculation with serum or virus of diseases, cause no more pain than the prick of a needle. Is that so ?*

A. Tennyson says that "a lie which is half a truth is ever the worst of lies," and I shall show you that "the prick of a needle" fraud is one of the worst connected with vivisection. If the matter ended with the initial operation there would be little to be said, but nobody needs to be told that the needle in question is a hollow pointed tube attached to a syringe, which is filled with the poison of certain diseases intended to be injected into the animal's blood.

*Q. The operation is somewhat similar to vaccination, I suppose ?*

A. Yes, but in vaccination the point of the lancet is the instrument used ; in bacteriological experiments the inoculating syringe takes its place.

*Q. What happens then ?*

A. This depends on the virus injected. Professor Crookshank, describing the results which follow inoculation with cultures of the disease-germ called *Micrococcus pyogenes aureus*, says : \*

"The micro-organisms injected into the pleura or knee of a rabbit, produce, as a rule, a fatal result on the following day ; but if it survives longer, it eventually dies of severe phlegmon [abscess]. If injected into the knee of a dog, suppuration occurs, followed by disintegration of the joint."

*Q. Are these effects very painful ?*

A. They cause acute suffering. Abscess of the knee-joint so extensive as to cause destruction of the parts would cause great agony.

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\* *Manual of Bacteriology*, by E. M. Crookshank. 3rd Ed. 1890. Pp. 246-7.

Q. *What effects do these disease germs cause when injected into other parts of the animal's body?*

A. Professor Crookshank says: "Injected into the peritoneal cavity [the abdomen] of animals they set up peritonitis [inflammation of the bowels], and introduced into the jugular vein they produce septicæmia [blood-poisoning] and death. When a small quantity of a cultivation was introduced into the jugular vein after previous fracture or contusion of the bones of the leg, the animal died in about ten days, and abscesses were found in and around the bones, and in some cases in the lungs and kidneys."\*

Q. *That seems to me about as great cruelty as it would be possible to inflict on any animal. Are such experiments often performed?*

A. They are the daily work done in all bacteriological institutes. Guinea-pigs fed with the contagion of foot-and-mouth disease have abscesses and ulcers. Sheep under a similar diet develop the disease itself.† The micrococcus of pyæmia injected into rabbits causes inflammation of the bowels and accumulations in the liver and lungs; in short, the appearance of blood-poisoning with abscesses.‡ The injection into rabbits of decomposing blood caused a spreading abscess to form which was fatal to the animal in about twelve days.§ Again the bacillus of jequirity contained in an infusion when inoculated into the eyes of healthy rabbits, produces severe inflammation of the organs.||

Q. *Inoculations into the eyes must cause real torture?*

A. Agony unspeakable hardly seems too strong a term to apply to it. When it is remembered that as in Dr. Cecil Shaw's experiments on "Sympathetic Ophthalmia,"¶ he irritated the eyes of rabbits for periods varying up to six months, it will be understood that this class of experiments affords as much scope for the operator's cruelty as he could possibly desire.

\* Becker, *Deutsche Med., Wochenschr.* November, 1883.

† Crookshank's *Manual of Bacteriology*. 3rd Ed., p. 235.

‡ *Ibid.*, p. 251.

§ *Ibid.*, p. 251.

|| *Ibid.*, p. 345.

¶ See Report in *British Medical Journal*, 18th June, 1898, p. 1583.

Q. *Do you imply that such experiments involve wanton cruelty ?*

A. If by "wanton" you mean "purposeless," I should say *No.* The researcher wished to determine what changes, if any, follow prolonged irritation of one eye, both with and without wounding ; as he had this object, which may be called a scientific one, his experiments were not "wanton," though they were certainly cruel in the extreme. That they were not performed simply to gratify the lust for tormenting animals does not absolve the operators from the charge of cruelly torturing them.

## CHAPTER XVI.

### BACTERIOLOGY.

*Q. It is often stated that the study of bacteriology has wrought a revolution in the Healing Art. Is that the fact ?*

A. It has revolutionised the scientific theories in pathology and created a new method of treatment of many diseases, but beyond enforcing the teaching that perfect sanitation is the only rational method of fighting the onset and progress of disease, it has really done very little for therapeutics or the art of curing our patients.

*Q. What is bacteriology ?*

A. It is the study of micro-organisms, that is to say, of minute living vegetable bodies termed bacteria, microbes, etc. Many of these are called pathogenic, that is to say, disease-causing organisms.

*Q. You say these minute organisms are vegetable structures, are they not often described as microscopic animals ?*

A. Yes, but that is incorrect. Bacteria and microbes, bacilli and micrococci belong to the vegetable kingdom, the domain of botanists. We may take yeasts, which are minute fungous ferments, as common examples of bacteriological life and action so far as the germ theory of disease is concerned.

*Q. Explain the germ theory of disease ?*

A. It is affirmed that certain diseases are always associated with the presence and multiplication in the body of characteristic micro-organisms, and these micro-organisms are the actual contagious causes of the disease. Diseases so caused are termed zymotic. So far as certain maladies are concerned, the proof is considered complete and capable of demonstration. Koch's chain of evidence is as follows :—

"1. The micro-organism must be found in the blood, lymph, or diseased tissue of man or animal suffering from or dead of the disease.

"2. The micro-organisms must be isolated from the blood, lymph, or tissues, and cultivated in suitable media, *i.e.*, outside the animal

body. These pure cultivations must be carried on through successive generations of the organism.

"3. A pure cultivation thus obtained must, when introduced into the body of a healthy animal, produce the disease in question.

"4. Lastly, in the inoculated animal the same micro-organism must again be found."\*

Q. *What are the chief diseases which are on these principles shown to be due to microbes?*

A. Fowl cholera, swine fever, foot-and-mouth disease, pyæmia, anthrax, tuberculosis, erysipelas, glanders, and tetanus.

Q. *How about scarlet fever, measles, small-pox, typhus fever, whooping cough, and influenza?*

A. The microbial origin of these diseases is only assumed, as no characteristic microbe has in their case been isolated with certainty.

Q. *Is the disease, when artificially produced, of the same character as the natural disease?*

A. There are wide divergencies, although the specific microbe can be demonstrated.†

Q. *May not the germ be the consequence and not the cause of the disease?*

A. Many competent writers have held that to be the case. The pathogenic or disease-causing organisms, as the bacteriologists call them, draw their nourishment from some living body which is their host and they are consequently *parasitic*, but they are also able to abstract nourishment from dead organic matter as is the case when they are cultivated in suitable media, they are then called *saprophytic*. Diphtheria and enteric bacilli, for example, thrive in filth as a culture medium. On this point Dr. Wilson says:—‡

"The whole trend of the influence of environment, or of 'soil and circumstance,' leads to the conclusion that, in respect to 'filth diseases' especially, the pathogenic properties of their characteristic organisms may be acquired in the transition from a life, which has hitherto been solely saprophytic [pertaining to a fungus developing in putrid matter], to a life which becomes parasitic [pertaining to an

\* *Manual of Bacteriology*. By Edgar M. Crookshank, M.B. 3rd edition, p. 17.

† *Handbook of Hygiene*. Wilson. 8th edition, p. 417.

‡ *Ibid*, p. 419.



organism infecting the body] and, therefore, specific. Though the organisms themselves are not generated *de novo*, their progenitors, so to speak, may never have had their habitat in man or animals; but so soon as they acquire an intensity, by means of their environment, to produce disease, and invade man or animals through the media of air, water, food, or by contact or inoculation, the disease has practically a new beginning in all such invasions or attacks. Granting, then, that there is 'no life without antecedent life,' the *de novo* origin of some of these diseases can be readily explained by this doctrine of microbial evolution."

*Q.* This seems a very reasonable and satisfactory solution of the difficulty between the supporters of the cause of disease and the consequence of disease theories. What I desire to know is how far the knowledge of these organisms and their life history has influenced the treatment of zymotic disease?

*A.* Dr. Wilson, in his great work from which I have already quoted, says: "There can be little question that the keenness of bacteriological investigation and experimentation on animal life have threatened of late years to ignore or minimise those factors of environment and dangers, which, from the view of practical sanitation, still remain of paramount importance, namely, pollutions or infections of air, water, and food, and reckless dissemination of contagia."\*

*Q.* I can understand that the enthusiasm of bacteriological research may blind the investigator to the importance of preventing disease by sanitary methods, but surely there must be some ground for the claims made by bacteriologists that their investigations have resulted in great saving of human life?

*A.* Let me answer this in the words of Dr. Wilson. He says:† "There are not a few who doubt whether all the agonies inflicted on animals sacrificed in the laboratories of continental workers in bacteriological research, or even in those at home, where the use of anæsthetics is enjoined, have saved one single human life, or lessened in any appreciable degree the load of human suffering."

*Q.* Is it not of great importance to the doctor to know the cause of the disease he is called upon to treat, and does not bacteriology throw great light on this?

*A.* Undoubtedly, and it has helped us to control the spread of infectious disease, but we have not realised the

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\* *Handbook of Hygiene*, p. 443.

† *Ibid*, p. 443.

great expectations of the possibilities of cure and protection that we were led to form in consequence of the adoption of this line of research.

Q. *What is meant by Serum therapy, toxins and anti-toxins in this connection?*

A. Serum therapy is the art of healing or protecting a patient by injecting into the system the fluid constituent of the blood of an animal which has been the subject of a zymotic disease, such as the patient has or is in danger of contracting. This serum is supposed to cure or protect the patient so treated in a somewhat similar manner to that which takes place in vaccination against small-pox. For example, the serum for diphtheria or anti-toxin consists of the serum or watery part of the blood of horses, previously rendered incapable of taking the disease, or of a principle called anti-toxin separated from it. These horses are freed from the risk of infection or immunised by the injection of the toxin or poison of diphtheria which is manufactured by the culture of the bacilli of diphtheria in broth. The horses are repeatedly injected with the clear liquid so that the serum of their blood comes to possess a high anti-toxic or anti-poisonous quality. The efficacy of the remedy is tested by injecting guinea-pigs with a dose of the serum at the same time that an injection of the poison of diphtheria is made into their system, this poison contains active bacilli of diphtheria. When the horse's serum is found to have reached the point when this combined injection of bane and antidote leads to no symptoms of diphtheria it is considered by the manufacturer to have acquired the requisite strength. The horse is then bled, and the serum prepared for use as an injection. The horse from which this serum is obtained is then found to be itself safe from infection with the bacilli of diphtheria. When this serum is added to a fluid containing virulent bacilli, they are declared to become harmless. The anti-toxin so prepared is now used for inoculating the human subject.\*

Q. *The serum, then, in this connection, is the watery portion of the blood of an animal who, having been, to a certain extent, poisoned with the virus of a disease and has recovered, is thereby rendered immune to it. The toxin is the poisonous ptomaine or putrefactive animal alkaloid and the anti-toxin is its antidote. Am I right?*

A. Quite right from the bacteriologists' side of the question.

\* This account of the method of preparing the diphtheria anti-toxin is adapted from *The Extra Pharmacopœia* of Messrs. Martin-dale and Westcott, 9th Ed., p. 471.



Q. *Are there any dangers connected with inoculation?*

A. They are many and serious. Pasteur's prophylactic against hydrophobia has undoubtedly caused hydrophobia in the patients who submitted to the inoculations. Koch's inoculations for tuberculosis, after rousing a frantic hope in the hearts of sufferers all over the world, ended in disappointment and disillusion. The professor said \*: "Calculating by body weight therefore, the 1,500th part of the quantity which produces no effects on the guinea-pig, has a very powerful effect on the human being." Dr. Koch also explained that:

"As regards the effect of the remedy on human beings, it was evident at the very beginning of the experiments that in one very important point the effect of the remedy on man is entirely different from that on the guinea-pig, which is the animal usually experimented upon. Here again is a fresh and conclusive proof of that most important rule for all experimentalists, that an experiment on an animal gives no certain indication of the result of the same experiment upon a human being."†

Q. *Is not Koch's tuberculin—although, as you say, utterly useless in the treatment of consumption—largely used as a means of testing for tubercular disease in animals?*

A. That is so; but Prof. Brown in his report of the Veterinary Department of the Board of Agriculture says: "It has been proved that one effect of inoculating animals with tuberculin has been in some cases that of distributing the tubercle bacillus to parts of the body not previously affected."

Q. *I notice that Mr. Stephen Paget says that "tuberculin (Koch's fluid) is now in general use for the detection of tuberculosis in cattle, to shut off the sources of the infection."‡*

A. The Department of Agriculture of the University of Aberdeen has published a report on an investigation concerning the value of this tuberculin as a test of the presence of tuberculosis in cattle. Out of 240 cattle tested in the investigation, 24 of those which gave no reaction to the tuberculin and were consequently pronounced to be healthy, were found, *post mortem*, to be tuberculous. §

Q. *Was any explanation given of that fact?*

A. The *British Medical Journal* tried to explain away the failure by suggesting that possibly the tuberculin loses its

\* *The Cure of Consumption*, by Prof. Robert Koch, pp. 8-9.

† *Ibid.*

‡ *Experiments on Animals*, p. 86.

§ See *British Medical Journal* (October 28th, 1899, p. 1213).

properties when kept for any length of time and may fail when the tuberculous mischief is minute or when the disease has become generalised. Any way, on a most carefully conducted research the test did fail and as the strength of a chain is no greater than its weakest link, we have a conclusive proof that there is very little dependence to be placed on this test.

Q. *What do the farmers think of it?*

A. Mr. John Thornton, in the *Live Stock Journal Almanac*, tabulates the results of the shorthorn sales in 1899. These results were quoted in the *Times* of December 5th, 1899. We read that:—

"The uncertainty of the tuberculin test has caused much vexation and disappointment, it being a delicate operation, requiring not only more time than is usually given to obtain the normal temperature of the animal, but great skill, and matured skill on the part of the operator. It is reported that similar difficulties and surprises have arisen in France, the home of its inception."

Q. *Mr. Paget says:\** "*The old tuberculin of 1890 that failed to cure patients who were already infected, succeeds in preventing the infection of healthy infants.*"

A. I believe that when the great disillusion about the cure for consumption came there were very large stocks of tuberculin left on the hands of the manufacturers and dealers, and it was necessary to find some use for them. It was a brilliant idea that although the stuff was worse than useless as a medicine it would do nicely for diagnostic purposes.

Q. *On the principle that it was a pity to waste it?*

A. Exactly. But I must explain what is meant by "preventing the infection of healthy infants." It is believed by most bacteriologists that the milk of tuberculous cows when given to children does in many cases set up consumption. To discover the presence of tubercle in the cow and then to prevent its milk communicating disease to children is the boon said to be conferred by Koch's fluid. But it has been objected that if all tuberculous cattle yielded tuberculous milk, the disease should be far more common amongst human beings than it really is. According to Lehmann and other German authorities, mixed milk, as usually purchased, is generally harmless so far as the spread of tuberculosis is concerned.†

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\* *Experiments on Animals*, p. 87.

† *Handbook of Hygiene*. By Dr. G. Wilson, p. 73.

Q. Mr. Paget says\* that "the antiseptic method was based on bacteriology, resting as it did on the proof afforded by Pasteur that putrefaction was caused by bacteria."

A. Lord Lister's antiseptic method of treating wounds has for its object the prevention of the development of bacteria in the wound by the use of chemicals, some of which destroy the bacteria and others merely prevent their growth. Carbolic acid and the carbolic spray were at first the routine agents employed in surgical operations by those who carried out Lister's practice. It was soon found that the carbolic acid and other chemicals did great harm, sometimes even poisoning the wound. At last Lister confessed that as regards the spray he felt ashamed that he should have ever recommended it for the purpose of destroying the microbes of the air.†

Q. Why did he say that?

A. He explained himself further in a speech at Liverpool, September 16th, 1896, when he said, "It was the graver forms of septic mischief, rather than microbes in the atmosphere, that we had to dread in surgical practice."

Q. How did the poor microbes get whitewashed?

A. It was actually discovered that some of them are remedial. Dr. Storer found that when old ulcers of the leg were sterilised by the microbes being killed by mercury the wounds did not heal by his oxygen treatment, but when the organism called *staphylococcus pyogenes aureus* (which the bacteriologists declared to be the cause of suppuration) was let alone the ulcers quickly healed, and the more abundant the microbes the sooner the patient recovered.

Q. What do the bacteriologists say to this?

A. Mr. Paget in his chapter on "Suppuration and Blood Poisoning," † after declaring that "all suppuration, and all forms of blood poisoning, abscesses, boils, carbuncles, erysipelas, puerperal fever, septicæmia, pyæmia—are due to minute organisms, various kinds of micrococcus," goes on to qualify his assertion by adding: "It has indeed been shown that suppuration may, in exceptional conditions, occur without micro-organisms."

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\* *Experiments on Animals*, p. 70.

† Address before the International Medical Congress at Berlin, August, 1890, reported in the "*British Medical Journal*," August 16th, 1890 (pp. 378-9).

‡ *Experiments on Animals*, p. 67.



## THE ANTISEPTIC TREATMENT.

Q. Tell me what surgeons themselves say about the antiseptic treatment?

A. To understand this you must first know what they mean by "Sepsis." "A septic process is one due to the activity of organisms [microbes, bacteria, &c.] which have only the power of developing in dead tissues or fluids, or in passive material such as blood-clot, pus, or serum."\*

Q. What happens when these organisms gain an entrance into the blood circulation?

A. They are quickly disposed of and do no harm, their activity is expended in causing putrefaction or fermentation. Many bacteria have these effects.†

Q. Is that all the mischief attributed to them?

A. They break up complex into simple compounds, and so disintegrate them. Then irritating or injurious chemical products are found and symptoms of septic poisoning supervene.

Q. What is the effect of all this on a wound?

A. Toxins are formed giving rise to inflammation of the surrounding parts, perhaps running on to suppuration, i.e., the formation of pus or "matter." When absorption of this takes place serious mischief often arises.

Q. How does all this arise?

A. From a dirty condition of the skin, dirty surgical instruments and dressings, or dirty hands. Not only practically innocent microbes may be present, but what are called by bacteriologists infective pyogenic organisms. All these effects are the terror of modern surgeons, and the minutest precautions are taken to prevent them.

Q. Of course they greatly retard the healing process and may cause the death of the patient?

A. Certainly. It is interesting to turn to a text-book of surgery thirty or thirty-five years old, and note the rise of the antiseptic system. For example, in Erichsen's *Science and Art of Surgery*, 5th edition, 1868, we find (Vol. I., p. 118) the following foreshadowing of Listerism: "The exclusion of air from a wound that it is intended to unite by primary adhesion is of the first importance. Whether air acts directly in

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\* *A Manual of Surgery*. Rose and Carless, p. 98.

† *Ibid*.

favouring the decomposition of the sero-plastic fluid that is effused on the cut surfaces, or whether, as Pasteur and Lister suppose, it is the medium by which organisms are conveyed by which preventive action is set up, in reality matters little; for the fact is certain that, unless it be excluded, suppuration will be set up." It is remarkable in contrast with surgical text-books of the present day how little was said forty years ago about the necessity of absolute cleanliness in surgical operations. We were told to remove dirt, pieces of stone, glass, &c., from the sides of the wound, but a wipe with a piece of wet lint or sponge was considered enough in those days. As for the surgeon's hands, his clothing, instruments, and dressings, they carried the danger of disease and death wherever they were present.

*Q. Tell me how the changes were introduced?*

A. It is claimed that the antiseptic method was based on bacteriological experiments "resting as it did on the proof afforded by Pasteur that putrefaction was caused by bacteria, and not by the oxygen of the air as had been previously believed."\*

*Q. To what surgical treatment did these bacteriological experiments lead?*

A. It led to a highly complicated system of antiseptic dressings for wounds with the idea of poisoning the germs in the air surrounding the patient by means of chemicals such as carbolic acid and perchloride of mercury. Carbolic acid was Lister's favourite application. He also introduced the carbolic spray, which filled the neighbourhood of the patient with an antiseptic steam, and was supposed to kill the spores in the atmosphere and so prevent putrefaction in the wounds.

*Q. Was not this dangerous to the patient's health?*

A. It often led to serious complications and carbolic acid poisoning was not infrequent. As I have already explained, Lister confessed that he was ashamed that he should have ever recommended the spray.

*Q. Do you mean, then, that the antiseptic treatment should be condemned?*

A. By no means. What I wish you to understand is that the claims made on its behalf are maintained on a wrong

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\* *Experiments on Animals.* Stephen Paget, p. 70.

basis. It is not the germs in the air that are the cause of the mischief to be combated, still less is their treatment by poisonous and irritating chemicals to be recommended. The whole credit of the system is to be given to the microscopic cleanliness, the absolute surgical purity which the treatment involved. Lister did much to reform surgical procedure in this direction, but all the experiments on animals failed to show him that nothing more was needed than the perfect cleanliness practised by every dairymaid, without chemicals, without sprays, without any ritual.

Q. *Can you prove this?*

A. I can. Turn to the article on Surgery in the last edition of the *Encyclopædia Britannica*, Vol. XXII., p. 691, you will find the objections made to the use of the carbolic spray and the following remarks:—"A few surgeons also without availing themselves of the antiseptic system appear to obtain as good, if not better, results than their fellows." Mr. Lawson Tait, the eminent abdominal surgeon, who was a pronounced opponent of vivisection and the antiseptic system, is stated in the article referred to as having performed 139 cases of ovariectomy in 1885 without a single death.

Q. *What precautions against septicæmia did he take?*

A. He used nothing but common tap water. He believed neither in the spray nor chemicals as a means of avoiding suppuration. The most perfect cleanliness in person, especially of the hands, absolutely clean instruments and dressings constituted what is known as the "Aseptic treatment."

Q. *What is the distinction between "Aseptic" and "Antiseptic"?*

A. "Aseptic" means free from septic matter. "Antiseptic" means preventing or destroying putrefaction. Dr. George G. Bantock, Senior Surgeon to the Samaritan Free Hospital, was frequently asked: "Would you open a knee-joint without an antiseptic?" His answer was: "Yes, most certainly, if the opportunity offered."\*

It has been proved that germs can live and flourish in the most aseptic of wounds, and sometimes delight themselves in solutions of carbolic acid.†

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\* *Provincial Medical Journal*, December, 1889, pp. 721-2.

† See *Anti-Vivisection Evidences*. B. Bryan, p. 40.



*Q. Has Lord Lister described any of his experiments on animals in connection with his discovery of the Antiseptic Treatment?*

A. He delivered a lecture at the opening of the Winter Session of Charing Cross Hospital Medical School on October 2nd, 1900, which was reported in the *Lancet* of October 6th, in which he described several experiments of a cruel character. To prove that inflammatory congestion may be brought about by nervous agency he says: "I may describe briefly the unpublished experiments with regard to it which I did. . . . One of the experiments was simply passing a silk thread through a fold of skin in a rabbit's back and knotting the ends together. . . . The other experiment was performed 20 minutes before the first, upon a part of the same animal which, being more sensitive, was more likely to show the effects of nervous disturbance. By means of a fine sewing needle I passed a delicate thread of silk through the left cornea and cut the ends off close with scissors. Next day the eye was much inflamed and its condition was aggravated on the day following, while the other eye remained healthy. As I have already said, the rabbit was killed on the expiry of 48 hours."

*Q. This must have been a very painful experiment. Was any anæsthetic used?*

A. There could not have been, as the inflammation progressed for 48 hours, a period too long for the animal to have been kept under its influence. Besides, Lord Lister says he selected the eye as the seat of his experiment, because being more sensitive it was more likely to show the effects of nervous disturbance.

*Q. Is the eye frequently chosen for similar experiments?*

A. Very frequently. For example in the *Supplement to the Nineteenth Annual Report of the Local Government Board for 1889-90*. Dr. Klein in his *Further Report on the Etiology of Diphtheria, Appendix B*, describes them as follows:—

"In last year's Report I have shown that with diphtheria membrane, a definite infectious inflammation of the conjunctiva and cornea" (of the eye) "leading to corneal ulcer, can be produced in the cat by rubbing a particle of the diphtheritic membrane over the cornea and conjunctiva, from which the surface epithelium has been previously scraped off. . . . Before the end of the week the cornea shows distinct ulcer. The corneal tissue becomes at the same time loosened and opaque, the conjunctiva much inflamed, cedematous, and at the internal canthus there collects muco-purulent matter."

. . . .

"During the year just passed" (presumably 1889) "a number of additional experiments on the cat's cornea and conjunctiva have been instituted with cultivations of this diphtheria bacillus. . . . I subjoin a few instances of successful inoculation of cats."

"1. With this cat's cornea bacillus . . . the scraped cornea of a fresh cat was inoculated.

"2. From a subculture . . . the scraped cornea was inoculated in two cats.

"3. From a culture, . . . the cornea and conjunctiva (previously scraped) of two cats were inoculated. In both these animals the disease set in with great intensity. . . . On the fourteenth day both eyes of this cat were closed, copious mucopurulent matter had collected. . . . This animal was killed on the seventeenth day; the disease still progressing. The ulcer on the cornea reached down to the Descemet membrane.

"4. From an Agar subculture, the cornea of two other cats were inoculated. . . . One of these cats was killed on the fifteenth day. One cornea was opaque, the other showed a deep crater-like ulcer, with raised opaque margin; conjunctiva much congested, swollen and coated with purulent matter. The other cat had considerable suppuration from the conjunctiva of both eyes.

"5. From a gelatine subculture, . . . the cornea of a cat was inoculated. On the fourth day there was a distinct ulcer on the cornea, with great congestion of the conjunctiva and mucopurulent discharge. The disease increased steadily until towards the middle of the third week, when there occurred perforation of the cornea, and the formation of staphyloma. The margin of the cornea had now become swollen and opaque, the conjunctiva bulbi, œdematous and deeply congested. The animal was killed about the end of the third week."

After this we hear of four other cats similarly diseased, the last being killed on the seventh day, with a "crater-like ulcer" on its eye.

*Q. Why are these experiments performed on the eye?*

A. We are told that the method "has the advantage that it enables the results to be observed day by day."

*Q. What defence do the experimenters make when charged with such obvious cruelty?*

A. They have the effrontery to say that such researches are "not necessarily painful." This alone should be enough to condemn them because it requires no technical training, no scientific education to enable anyone to judge correctly on such a question. Everybody has felt the pain and distress caused by the presence of the smallest particle of dust in the eye. Yet an apologist for Lord Lister's experiment on the eye of the rabbit to which I have referred



wrote to the *Daily Chronicle* (October 22nd, 1900) contending that the presence of stitches in the eyeball with consequent inflammation and congestion is not necessarily painful.

*Q. It seems incredible that apologists for the torture of animals should maintain so preposterous a proposition?*

*A.* There is nothing they will not maintain to repel the charge of cruelty. It is all of a piece with the contention that mutilation of the brain, the bowels, and inoculation experiments do not entail pain. Everything with which the friends of animals charge their tormenters is denied, because to admit that cruelty is practised in the laboratories would be fatal to the continuance of vivisection in England.

## CHAPTER XVII.

## PASTEURISM.

*Q. I presume that what is known as " Pasteurism " is another name for bacteriological research ?*

*A.* Much the same so far as our question is concerned. In 1857 Pasteur demonstrated that lactic, acetic, and butyric fermentations were produced by micro-organisms. But Schwann (1810-1882) discovered the influence of the lower fungi in causing fermentation and putrefaction, so that he may be called the father of the germ theory of disease. In popular literature, however, the credit of the new science of bacteriology is generally conceded to Pasteur and his school at Paris.

*Q. Are many animals used there ?*

*A.* Immense numbers. There is a great menagerie attached to the Institute where all sorts of animals are to be seen passing through the various stages of illness caused by inoculations with every kind of malignant virus. The place is a veritable " Hell of the Animals."

*Q. A journal is published there, I believe ?*

*A.* Yes. It is called *The Annals of the Pasteur Institute*. It is published in French.

*Q. Does it detail the different steps of each research undertaken there ?*

*A.* It does. For example, in the number for December, 1899, there is the record of " Researches on the Bacteriology of Ozæna." Ozæna is a fœtid ulceration of the nose and discharge which is attributed to a bacillus called Löwenburg's diplococcus by some Pasteurian researchers. Dr. Fernand Pevez, physician to the Children's Hospital and the French Hospital of Buenos Ayres, performed a number of experiments on various animals.\* In the history of the inoculations we

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\* Travail du laboratoire des éleveurs, dirigé par M. J. Liguères, d'Alfort, at the Institute, in the search for the organism supposed to be the cause of the disease. The experimenter says: " We have many times sown our cocco-bacillus in the noses of rabbits and monkeys. The natural penetrating odour of these animals has prevented us from distinguishing the peculiar smell of the microbe. Otherwise its development in the nasal fossæ has been very well marked."

read that when guinea-pigs were inoculated subcutaneously the process was followed by a hot and painful swelling, which increased daily, and ended by breaking, give vent to thick pus. The evolution of this abscess is sometimes very long.

Q. *Were the inoculations made in the bowels?*

A. Yes. We learn that after death following such experiments there was found very intense inflammation, with very abundant bleeding spots. The victims must have suffered cruelly.

Q. *What effect had the inoculations on rabbits?*

A. At the seat of inoculation "a tumour appeared, which was hot and painful," and rapidly attained to the size of a pigeon's egg. Under the skin of the ear the inoculation produced "inflammatory accidents" of erysipelatous character, and the ears became much swollen. In one of the animals inoculated 30th November, 1898, the temperature rose rapidly, the creature became dull and panted for breath, and lost its appetite. A purulent secretion ran from the nostrils, the emaciation became extreme, and it died in the night of 10th to 11th December, 1898. The *post-mortem* examination showed that the spleen was enormously enlarged, there was inflammation of the sac around the heart, inflammation of the lungs, congestion of the windpipe, and other painful complications.

Q. *Was that case peculiar?*

A. Not at all. Another case is recorded, however, which was termed "remarkable and presents a very great interest." This animal was much affected by the inoculation; there was high fever, prostration, and complete loss of appetite. The next day the nose began to run and a purulent discharge came on which lasted for 15 days. The animal was extremely emaciated, death appeared imminent. To the great surprise of the experimenters, however, it began to improve, the appetite returned. A month later the rabbit ate as if quite well, at the same time they observed at the side of the ears "some very interesting phenomena." The two ears were swollen, enormous, hot and painful. At the edges appeared livid bluish spots, which became black and cold, and presented the appearance of dead tissue. After the renewal of the diseased structures the edges presented great notches. We remark that these lesions of the ears are bilateral, although the inoculation was made on only one side. Four months afterwards this rabbit appeared quite well, then a new accident happened. On the back of the nose, almost at the

point, appeared a little projection, which grew till it became the size of a hazel-nut. It became a cold abscess, lasting a month. From this, a white thick matter was taken, which gave an almost pure culture of the bacillus of ozæna, this took place five months after the inoculation without apparently affecting the general health. The 15th of August, 1899, the animal was killed.

*Q. I presume that it was the recovery of the animal that was so curious?*

A. Exactly. Many other experiments of the same kind are recorded. One animal inoculated in a vein died in 24 hours. Another was dying for two days, it ate nothing, the ears swelled up, the creature remained always lying on its side, they killed it. A young monkey was inoculated in the nose by the aid of a spatula charged with the same culture. An irritation was produced, the animal began to sicken, and died after two months.

*Q. And all this I suppose is euphemistically described in England as "merely inoculation experiments?"*

A. I have detailed these cruelties, which are quite everyday matters in bacteriological laboratories, that you may know what importance to attach to the oft-repeated phrase used by apologists for experiments on animals in describing inoculation experiments as "causing no more pain than does the prick of a needle."

*Q. Would not such effects as you have described often be really more cruel than many cutting operations?*

A. Certainly. Yet bacteriological experiments which daily become more numerous as Pasteur Institutes spring up all over the world, are minimised in the most deceptive manner, and it is highly important that the public should understand something of the great cruelties practised within the walls of such places.

*Q. I am told that Pasteur discovered the cure for hydrophobia. Is that so?*

A. Like many other persons, you have been misinformed. Pasteur never pretended to cure hydrophobia. When that terrible disease has once set in he never suggested that his method could in any way help the patient. What he did try to prove was that by his method of inoculation he could prevent an attack of hydrophobia in a person bitten by an animal supposed to be rabid.



Q. *But surely that is not susceptible of proof?*

A. Of course not. First, it is not easy to say in a given case that the animal inflicting the bite was rabid, because a dog, for example, suffering from pain may appear to be "mad" when it is not. Then again, in a number of persons bitten by an animal really rabid it is difficult to say how many will contract hydrophobia. Very much depends upon the locality of the injury. If a rabid animal bites through clothing it is highly probable that the virus will be rubbed off the teeth before the wound is inflicted. We can seldom be sure the dog was mad, or that if mad and did not bite the victim's naked flesh, that the virus reached the blood of the patient nor can we be certain that even the bite of a rabid animal will be fatal in more than from five to ten per cent. of the cases.

Q. *I suppose that in many cases the patient has been needlessly alarmed by a perfectly innocent animal?*

A. Mr. Victor Horsley, F.R.C.S., M.B., F.R.S., said :—\*

"After the first few months in which M. Pasteur practised his treatment, he was occasionally obliged in order to quiet fears, to inoculate persons who believed they had been bitten by rabid animals, but could give no account of it."

Dr. T. M. Dolan said :—†

"M. Pasteur has treated a large number of persons bitten by non-rabid dogs. He has also treated a number who were not even bitten, but who had been licked by dogs. He has even injected with his bouillon persons never bitten, but who submitted to the process through curiosity."

Q. *Does not a post mortem examination of the body of the suspected animal settle the question of the madness?*

A. No. M. Colin, Professor at the French Veterinary School at Alfort, said :—‡

"The post mortem examinations of the dogs are equally valueless ; they afford no evidence of the madness of the dogs. The only way of arriving at a certain conclusion is by the prolonged observation of the animal, which should be shut up and kept till the characteristic symptoms declare themselves. It is, therefore, evident that a great number of the persons reported to be under the influence of hydrophobia were bitten by dogs that were never mad at all."

It is impossible to diagnose rabies by simple post mortem examination.

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\* Report of Local Government Board Committee, pp. IV., V.

† Letter in *British Medical Journal*, September 4th, 1886.

‡ Speech before the French Academy of Medicine, Paris, November 9th, 1886.

Q. *But cannot the question be settled by testing a decoction of the brain or spinal cord of the suspected animal on a rabbit by inoculation?*

A. This is done but is by no means conclusive. "The death of a rabbit with cerebral symptoms," says the *Veterinary Record*,\* "is not a positive indication of death from rabies."

Q. *Why is this so?*

A. "M. Vulpian injected under the skin of rabbits saliva collected at the very moment of the experiment, from perfectly healthy individuals, and this injection killed the rabbit so inoculated in forty-eight hours. The blood of these rabbits was found to be filled with microscopic organisms; among which was a special organism discovered by M. Pasteur in the course of his experiments with inoculation of the saliva of a child who had died of rabies."†

Q. *That reminds me to ask you whether M. Pasteur discovered the microbe or organism which is supposed to cause rabies?*

A. No such organism has been discovered. The *Standard* (April 7th, 1897), put the matter so lucidly that I cannot do better than quote it:—

"Nothing is known with exactitude about the nature of the poison [of rabies] although it has been constantly studied for many years in the best laboratories in the world. If it be bacterial the microbe has defied detection, and its life history is, consequently, a matter of pure conjecture. In the case of other bacteria, such as that of cholera, which has been completely identified and investigated for years under every condition, natural and artificial, the highest authorities have found, to use Professor Grüber's words, that the more deeply it is studied the more mysterious does it become. How much less, then, can be understood about a microbe which has never been seen, and is only supposed to exist. For all we know, it may be latent in the air, in the ground, in water, or in the bodies of dogs and other animals. To pretend to any scientific certainty about it is to throw dust in the eyes of the public."

Q. *Am I to understand then, that Pasteur never discovered what is the cause of rabies?*

A. He could never discover it, nor did he know how his so-called protective inoculations act.

Q. *Describe his method.*

A. In 1884 M. Pasteur announced at the Medical Congress at Copenhagen that he had discovered a method rendering

\* May 2nd (and 16th), 1896, p. 612.

† *British Medical Journal*, April 9th, 1881, p. 571.

dogs refractory to rabies. He said that the virus used for his inoculations was strengthened by being transmitted through rabbits, and weakened by being passed from monkey to monkey.\*

"At first it was in the brain that the virus was to be obtained in perfect purity; then trephining with intradural inoculation was the sovereign method; then intravenous inoculations were said to simplify the matter; then blood was a good virus; then smaller quantities produced fiercer rabies; then inoculations in series modified the virus after many variations; then a few monkeys and rabbits did the work; then rabbits alone sufficed, while the virus was weakened by drying the cord. And, to crown all, forgetting the traditions of his own work in regard to charbon and chicken cholera, Pasteur said that the protective character of his virus depended upon a reduction in quantity and not in the virulence of the virus.†

Q. *How is the anti-rabic virus prepared for the inoculations?*

A. Dr. Sims Woodhead, Director of the Laboratories of the Royal Colleges of Physicians and Surgeons in London, has described the process very fully in his work on *Bacteria and their Products*. Pasteur found that the saliva of a child developing hydrophobia after the bite of a mad dog when inoculated into a healthy rabbit killed the animal in two days. Taking some of the saliva of this animal he treated another rabbit in the same way with a like result. This was the beginning of the Pasteurian inoculations in this line of research.

Q. *What was the next step?*

A. It was discovered that the fluid from the brain and spinal cord (cerebro-spinal fluid) and fragments of the brain and spinal cord of a hydrophobic rabbit introduced under the covering of the brain would produce the disease in healthy rabbits.

Q. *What resulted from this?*

A. Pasteur found that the virus when propagated through a series of rabbits rapidly increases in its virulence. Dr. Osler‡ says that:—

"Whereas subdural inoculation from the brain of a mad dog takes from fifteen to twenty days to produce the disease, in successive inoculations in a series of rabbits, the incubation period is gradually

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\* *Medical Times*, August 23rd, 1884.

† *Dr. Dulles, Address to Philadelphia Medical Society*, January, 1886.

‡ *The Principles and Practice of Medicine*, 2nd Edition, p. 179.



reduced to seven days. The spinal cord of these rabbits contain the virus in great intensity, but when preserved in dry air the virus gradually diminishes in intensity. If now dogs are inoculated with cords preserved for from twelve to fifteen days, and then with cords preserved for a shorter period, *i.e.*, with a progressively stronger virus, they gradually acquire immunity against the disease. A dog treated in this way will resist inoculation with material from a perfectly fresh cord from a rabid rabbit, which otherwise would inevitably have proved fatal. Relying upon these experiments, Pasteur began inoculations in the human subject, using, on successive days, material from cords in which the virus was of varying degrees of intensity."

*Q. What is the "bouillon" used in the inoculations?*

A. It is a broth or liquid nutritive medium for culture purposes. Pasteur on the 26th of October, 1885, described his method to the French Academy of Sciences. He took fragments of different cords, each beaten up with twice its weight of sterilized bouillon, commencing with the weakest virus, and inoculated animals with it on ten successive days. He found that they were protected against hydrophobia even when extremely virulent virus was afterwards injected under the skin or into the membranes of the brain. It was after his experiments on fifty dogs that he began experimenting on human beings.

*Q. Who was his first patient, and what happened to him?*

A. The first human being inoculated by the Pasteurian method against hydrophobia was Joseph Meister, a boy aged nine years, who was severely bitten on the arms and legs by a mad dog on July 4th, 1885. He was attended to by a doctor who cauterized the worst of the wounds with carbolic acid, twelve hours after the child had been bitten.

*Q. I have heard that efficient cauterization is itself a perfect preventive against hydrophobia? Is that so?*

A. Pasteur himself declared this to be sufficient. A letter was written by him to a doctor near Paris in the following terms: "Sir,—The cauterizations that you have practised ought to re-assure you fully as to the consequences of the bite. There needs no other treatment; it is useless.—L. Pasteur."

*Q. Did Meister die?*

A. No. He was inoculated 13 times in ten days, and his survival was claimed as proof of the efficacy of Pasteur's treatment. Dr. Dulles, commenting on this case, said: "A full half of the spinal cords used in the crucial experiment on

Joseph Meister . . . . proved to have no virus when tested on rabbits."

Q. *Is it certain that the dog was mad?*

A. The evidence of this was very slight, being confined to the fact that in its stomach were found bits of straw and stick.

Q. *Cannot such a point be settled by post-mortem examination of the animal?*

A. It cannot; it is impossible to diagnose rabies by simple *post-mortem* examination, as I have already explained.

Q. *Is not the inoculation against hydrophobia very much like vaccination?*

A. It is often said to be similar, but this is not the fact. Dr. Bell Taylor\* has very lucidly explained the difference between the two methods. He says:—

"By vaccination we give the patient something which is considered equivalent to small-pox in a mild form, and so place him in the position of one who has had the disease; and if there was the slightest reason for supposing that hydrophobia was a disease of this kind—if rabies in the dog was the same disease as hydrophobia in the man—if the patient was inoculated before being bitten—if there was the slightest ground for concluding that a patient who had had hydrophobia once could not have it again (which there is not), and if M. Pasteur, by his inoculations, could cause hydrophobia in a mild form, and so protect his patients against a fatal attack—then the comparison which is so often made between his injections and Jenner's system of vaccination would hold good. But M. Pasteur's injections produce no effect whatever unless they cause hydrophobia, and when they do cause hydrophobia the patient usually dies.

"If we vaccinate a patient who has not been vaccinated before, and the vaccination does not take—if no constitutional disturbance results—if there be no rise of temperature, no swelling, no redness, no pain, no formation of vesicles containing inoculable lymph—then the patient is certainly not protected, but is in precisely the same condition as one who has not been vaccinated at all; and if we subject a patient who has been bitten by a mad dog to a series of hypodermic injections that produce no effect whatever, no constitutional disturbance, no fever, no hydrophobic symptoms, no local irritation, then he is clearly just as likely to die of hydrophobia as he would be if nothing had been done."

Q. *Have Pasteur's inoculations ever caused hydrophobia?*

A. Certainly they have. Professor Michel Peter, the distinguished physician of Paris, said years ago: "M. Pasteur

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\* *Pasteur's Prophylactic*, from the *National Review*, July, 1890.

ne guérit pas la rage, il la donne"—"he does not cure hydrophobia, he gives it."

Q. *Do statistics bear out this charge?*

A. They do. The report of the French Conseil Supérieur de l'Hygiène shows that "from 1850 to 1885 the average annual mortality from hydrophobia in France was 23; from 1885 to 1890 inclusive, after Pasteur started his inoculations, there was a yearly average of 39 deaths in the same country, and under precisely similar conditions." "In England the deaths from hydrophobia from 1880 to 1884 inclusive, were 153, while those from 1885 to 1889—years during which many persons bitten by dogs were sent from this country to Pasteur—were 159, giving a full addition of one to the yearly average."

Besides this, we have the records of no less than 1,220\* deaths from hydrophobia of persons who had submitted themselves to the Pasteur inoculations and were supposed to be thereby perfectly protected from the results of their injuries.

Q. *This shows that the system does not cure, it hardly proves that it causes hydrophobia.*

A. Professor Peter said that at least one patient, named Pierre Rascol, certainly died after inoculation from *rage du laboratoire* or Pasteur's rabies. Besides patients bitten by dogs pronounced not to have been mad, have died of hydrophobia after undergoing the preventive inoculations.†

Q. *Have you visited the Pasteur Institute in Paris, and seen the inoculations in progress?*

A. I have. I visited also the vast menagerie of animals kept for experiment and for supplying the virus for the injections. I saw hundreds of rabbits who had been inoculated with the virus of rabies. The disease does not act on rabbits as on dogs. Rabbits do not bite. Some days after inoculation they become paralysed in the hind quarters; they are unable to move, but lie helpless in their cages and slowly die.

Q. *Did you see dogs in the menagerie?*

A. Yes, and heard them long before I reached the great cage, behind the strong iron bars of which the creatures were

\* Up to February, 1901.

† See *The Pasteur "Cure" for Rabies*, with the names and data of 1,220 persons who have died of hydrophobia after the Pasteurian treatment. Published by the National Anti-vivisection Society, London, February, 1901.

imprisoned. I saw about a dozen beautiful dogs, raging furiously at the bars, frantic to be free.

*Q. Has it not been denied that dogs are used in the preparation of the fluid for the anti-rabic inoculations?*

A. Yes, but it is incontestable that they are so employed. Here is a description by an eye-witness of this fact:—

"M. Pasteur . . . took us into the cellars of his laboratory. There, in circular cages of close trellis work, are imprisoned the dogs of different kinds. One of them has arrived at the last stage of rabies. He cannot bark in a natural manner, but emits hoarse and characteristic cries somewhat like the crowing of a cock. These peculiar cries frighten the occupants of the neighbouring cages, and they would certainly escape at full speed if M. Pasteur allowed the doors to be opened. If one kicks the door of a mad dog's cage, he rushes to the trellis work and gnaws it furiously. A thick bar of iron is held to him; he seizes it in his mouth, grinds his teeth upon it, and it is difficult to wrest it away from him; the same thing occurs when the end of the bar has been previously heated."\*

An article appeared in the *Fortnightly Review*, July, 1886, in which appear the words "the delirious period is fraught with mental and physical torture to the trepanned dog," that is to say, to a dog whose skull is bored and has the virus of rabies injected into the brain. In the *Daily Graphic*, September 2nd, 1890, the reporter describing a visit to M. Pasteur's new laboratory, says that he was shown by the guide "quite a nice assortment of mad dogs."

*Q. Have many experts condemned the Pasteurian inoculations against hydrophobia?*

A. Professor Peter, of Paris, whom I have already mentioned, declared the system to be altogether empirical and devoid of scientific basis. Before the Paris Academy of Medicine he preferred a crushing indictment of the system, and produced a mass of incontrovertible evidence in support of his terrible charge that several of Pasteur's patients had died of a form of hydrophobia almost unknown hitherto in the human subject, but very closely resembling the disease produced in rabbits by Pasteur's inoculations.

*Q. Several German professors of great repute, I believe, have condemned the system?*

A. Yes. Professor Billroth declared it to be a *fiasco*, and Professor von Frisch, of Vienna in a very exhaustive criticism and report of Pasteur's treatment, said:—

"Rabbits and dogs, which, without preceding infection, were subjected to the last-mentioned strengthened inoculation for human

\* *La Paix*, August 15th, 1882.

beings, were infected with rabies through that inoculation. Hence it may be inferred, with great probability, that this method of inoculation may likewise be seriously dangerous to man."

*Q. Professor Billroth is the great Viennese surgeon, is he not?*

A. Yes, and there is no greater authority in surgery. In a review of a criticism of Pasteur's method by Professor von Frisch, of the Bacteriological Laboratory of the General Polyclinic at Vienna, he says:—

"The Viennese public, like the rest of the world, was carried away by the spell of the new discovery, but soon the need was felt to see more clearly into the matter. A highly-gifted lady, full of enthusiasm for all that is good and great, procured the necessary funds on behalf of the 'General Polyclinic' (the well-known association of University professors) to test the question on a large scale. Through a most auspicious circumstance there happened to be an active member of this institute, who had long since made a name by his experimental and bacteriological labours, Professor A. von Frisch, who was accordingly sent by the Polyclinic to Pasteur at Paris."

*Q. Was not that a rather remarkable proceeding?*

A. It was, and Dr. Billroth expresses his surprise that such a course should have been necessary and explains how this was so. It was due to the fact that M. Pasteur had shrouded in mystery all his utterances concerning his system and manipulations:

*Q. Were his descriptions so inexact?*

A. His descriptions were so incomplete and were put before the scientific world in a manner so contrary to the established usages amongst scientific men that it was impossible for the doctors at Vienna "to work according to his haphazard method."

*Q. How did Pasteur receive von Frisch?*

A. In a very cordial manner, but he only communicated his discoveries aphoristically.

*Q. Possibly he thought that von Frisch was prejudiced?*

A. That was not so, he always spoke of Pasteur's labours "hat-in-hand," but he came to the conclusion, after his experiences in Paris, that "the great hopes which the medical world had placed upon Pasteur's discovery had not been realised."

*Q. How long was von Frisch engaged in this research?*

A. He continued his labours in this connection in the most extensive and minute manner possible, night and day, for

more than a year, by the assistance of Drs. Francisci and Eder, and with the aid of unlimited material help from the Polyclinical Institute.

Q. *With the result you have stated just now?*

A. Yes, that the method of inoculation may be seriously dangerous to man. Deputy Surgeon-General J. H. Thornton, C.B., M.B., M.A., in a pamphlet on *Rabies and Hydrophobia*, says, p. 10:—

“Dr. Charles Bell Taylor, of Nottingham, in his article in the *National Review* of July, 1890, gives the following cases which furnish decisive proof that hydrophobia is sometimes brought on by the Pasteurian inoculations. Leopold Nee was bitten at Arras on November 9th, 1886; he was subjected to the Pasteurian treatment on the 17th and following days, and died of hydrophobia on December 17th, a month later. *The dog that bit him was perfectly healthy.* In July, 1887, Arthur Stoboi, one of the scholars at the Lyceum at Lublin, in Russia, was bitten by a dog, and was immediately sent to the Pasteur Institute at Warsaw, where he received the usual treatment by inoculation, and was discharged on August 11th with a certificate of cure, on the strength of which he was re-admitted to the Lyceum, and resumed his duties. On November 9th, however, three months later, he felt pain in the region of the inoculations, and shortly afterwards he died of hydrophobia. *The dog that bit him remained quite well.* The groom of Signor Camello Mina was bitten by a sheep dog, and subsequently died of hydrophobia after having been subjected to the Pasteurian treatment at Milan for a month. *The dog had nothing whatever the matter with it.* A young painter of Antwerp, named De Moens, when visiting a friend, was bitten slightly by his friend's dog. He was urged to go to M. Pasteur at once, which he did, and was subjected to the Pasteurian anti-rabic treatment from March 20th to April 2nd, 1889. After his return he was suddenly attacked by hydrophobia, and died May 17th, 1889. *The dog that bit him remained perfectly well.* It is quite evident that these persons died from hydrophobia, communicated to them by the Pasteurian anti-rabic treatment, and I challenge Lord Lister, Mr. Paget, and the other advocates of Pasteurism to explain these cases otherwise if they can.”

Q. *Why do not the Pasteurian inoculations kill the patients in greater numbers if they are so dangerous?*

A. For the reason that the virus used in the inoculations is generally so weak as to be harmless, and so they produce no effect in the majority of cases. What was known as the “intensive” method of inoculating has long since been given up. It was too manifestly fatal. To go down to posterity as the discoverer and propagator of a laboratory disease was not only to jeopardise one's reputation as a man

of science, it was coming too near the tactics of an exterminator of one's species.

Q. *Was it really so bad as that?*

A. In the report to our Local Government Board, which was presented to Parliament in 1887 and which dealt with the Pasteurian Treatment of Hydrophobia, the Committee say that "after the intensive method, deaths have occurred under conditions which have suggested that they were due to the inoculations rather than to the infection from the rabid animal."

Q. *Did not Pasteur once compare his inoculation for rabies to an express train which overtakes and passes the Parliamentary train represented by the slowly-developing hydrophobia in the patient's system?*

A. He did, but when one train overtakes another on the same line of rails the result is usually disastrous.

Q. *Now tell me something about Pasteur's statistics. We see in the papers from time to time wonderful reports of the way in which the inoculations prevent bitten persons from developing hydrophobia. Are those statistics of any value?*

A. They are of no value, nor is it possible to make them so, and for these reasons: The persons who go to the Pasteur Institute for inoculation have no symptoms of disease except a more or less serious bite or scratch from an animal which may or may not have been rabid. They seek treatment as a precautionary measure only. They have possibly incurred some risk of hydrophobia, just as any resident at an English seaport town incurs a certain risk of taking the plague if a patient arrives in the port suffering from that disease. Let us suppose that there is in that port an institute for inoculating people against the plague and ten thousand of the inhabitants undergo the treatment for its prevention, would it be reasonable to say that the whole of the ten thousand inhabitants who were so inoculated had been saved from the plague?

Q. *Certainly not, because there was but the very remotest danger of their taking it, but is the illustration quite on all fours with the rabies prevention?*

A. I think it is. Tens of thousands of persons are bitten or scratched in England every year by animals that nervous and ignorant people consider to be mad. Thousands of people attend the hospitals, chemists and doctors, and have their wounds cauterised in such cases, yet hydrophobia is the rarest of diseases in this country. Would it be fair of the



mended in the *Extra Pharmacopœia* (Martindale & Westcott, 9th edition. Therapeutic Index, p. 616, art. "Hydrophobia"). Again, take the Synoptical Index to the *Medical Annual*, 1887 to 1898, turn to "Pilocarpine," p. 67. We find it recommended for "Rabies," and the reference is given to the volume of the *Medical Annual* for 1893, p. 454, where we read: "In the November *Indian Medical Gazette*, Assistant-Surgeon Troylucho Nath Ghose reports a case of rabies cured by eleven subcutaneous injections of Pilocarpine Hydrochloride,  $\frac{1}{2}$  of a grain each, in the course of seven days."\*

There is nothing therefore "idiotic" in the vapour bath treatment as Mr. Paget suggests.

Q. *What has he to say about Dr. Buisson's cure?*

A. He says: "It was an ordinary case of fear of hydrophobia," in other words, that Dr. Buisson had nothing the matter with him, and was "cured" by suggestion.

Q. *Do not many patients of the most orthodox physicians and surgeons often get cured in a similar way?*

A. It is quite certain that multitudes of Pasteur's patients do. The worst of it is if they have nothing the matter with them but "ordinary fear of hydrophobia," they run a terrible risk of contracting the genuine thing by submitting to the inoculations. Mr. Paget calls the Buisson system "arrant nonsense." The Pasteurian inoculations are certainly not to be described by any such contemptuous terms.

Q. *Can you give me the history of other cures by the Buisson treatment?*

A. Surgeon-General Thornton publishes a number of them in the book from which I have quoted. He says:—

"I totally disagree with Mr. Paget, and, on the contrary, consider that it was undoubtedly a case of true hydrophobia. Other cases are on record proving the efficacy of this treatment, even when the disease is fully developed. The following are some of these cases:—

"A boy was bitten by a rabid dog at Kischineff, near Odessa, on April 8th, 1886. On April 25th the boy showed signs of hydrophobia, being unable to swallow liquids, the sight of which induced paroxysms. The Buisson treatment was resorted to, and the boy recovered in a few days.

"At the Industrial School, Arlington, New Jersey, a lad named Klee was bitten by a rabid dog on January 2nd, 1886; on January 22nd symptoms of hydrophobia set in, and on the 24th were

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\* Reference—*New York Medical Journal*, January 2nd, 1892.

fully developed. The vapour bath treatment was employed, and the patient was cured in a short time.

"A little girl named Pauline Kiehl, in Paris, was bitten by a dog which must have been rabid, as two other persons bitten by it both died from hydrophobia. She was refused treatment by M. Pasteur, who probably regarded the case as hopeless, a considerable interval having elapsed between the infliction of the injury and the application for prophylactic inoculation. Hydrophobic symptoms were already setting in when the child was taken to the establishment of Dr. Léon Petit, 108, Faubourg St. Honoré, Paris, where the Buisson treatment was carried out. She underwent it and recovered. This case is alluded to by Mr. Paget, who declares that the refusal of treatment by M. Pasteur is the strangest feature of the case. On the contrary, it was Pasteur's invariable rule to have nothing to do with any case where hydrophobic symptoms had appeared, and it would have been very strange if he had not refused.

"Mr. Paget is very sarcastic about 'that mass of cures in Asia,' but without any reason that I can see, unless it be to delude the public. Here is a case reported by Dr. Scott, of the Jaffna Hospital, Ceylon. A patient who had been bitten by a dog eight days previously was brought to the hospital in a state of severe mental anxiety. No definite symptoms of hydrophobia were present, but a vapour bath was given, and repeated the same evening. Next day he seemed much better, but on the twelfth day after the bite definite symptoms of hydrophobia appeared. The spasm of the muscles of deglutition and respiration was very well marked, and the anxiety on the face of the patient was most pitiable. He was given a vapour bath for half an hour, and this was repeated several times during the next three days. All the hydrophobic symptoms disappeared, and he went home quite well in a short time.

"The following cases furnish satisfactory evidence of the efficacy of the Buisson treatment in preventing the development of hydrophobia in persons bitten by rabid animals :—

"On May 6th, 1895, Dr. Ganguli, of Dinajpur, India, successfully applied this treatment to four persons who had been bitten by a rabid jackal on April 29th; while another man, bitten by the same animal, who did not undergo the treatment, died of hydrophobia.

"In April, 1896, Dr. Dass, of Narainganj, India, employed this treatment successfully in the cases of two persons who had been bitten by rabid dogs more than 14 days before. In each case several other persons bitten by the same dog, who did not undergo the vapour bath treatment, died of hydrophobia.

"In 1896, Mr. J. H. Kotwal, of Bassein, was bitten by his own bulldog, which was proved to be rabid. For a fortnight he underwent the Buisson treatment, and no bad result ensued. Two other persons bitten by the same dog subsequently died of hydrophobia, having had no treatment.

"Now, in spite of Mr. Paget's sarcasm, I maintain that the foregoing instances furnish ample evidence of the beneficial effects of the vapour bath treatment in preventing, and even in curing, hydrophobia; and it is difficult to understand why this simple and

harmless, yet effectual method of treatment, devised by a fully qualified medical man, has been treated with such indifference and neglect by the medical profession at large, while at the same time they have been quite ready to accept an unsound and dangerous system of treatment, invented by a man who was neither a physician nor a surgeon, but a chemist, and whose anti-rabic system is downright quackery."

*Q. Yet in face of all this evidence do I understand that patients are allowed to die in our hospitals without anything of the kind being tried?*

A. I know of no cases where the vapour baths have been tried in our hospitals. I have always considered the system boycotted. Certain drugs are used, as curare to stop the spasms, chloroform and nitrite of amyl, but the evidence is all against their value.

*Q. What is Tetanus?*

A. It is a disease with spasmodic and continuous contraction of the muscles. When the muscles of the lower jaw are affected in this manner it is called "locked-jaw."

*Q. It is a very fatal disease, I believe?*

A. Yes. Death usually occurs between the third and fifth days; partly from suffocation, partly from exhaustion.

*Q. Is it true that it is caused by a bacillus?*

A. When the convulsions arose from poisoning associated with an open wound, surgeons were led to believe that it was due to infection. Dr. Sims Woodhead says:—\*

"That this poisoning was the result of the activity of an organism, which made its way to a wound and there flourished and gave rise to the characteristic products and symptoms, was not the result of direct experiment made with the object of finding out such an organism, although attempts were not wanting to demonstrate its presence in the wounds and along the course of the nerves in cases of tetanus. All efforts, however, proved unsuccessful until after an organism obtained from other sources had been obtained and described, and an artificial tetanus had been produced."

*Q. How is it supposed that the bacillus gets into the wound?*

A. When soils obtained from the streets and from fields were inoculated into mice, rabbits and guinea-pigs, it was found that symptoms like those of tetanus were produced. The animals became paralyzed and the whole body became rigid. After death there was found at the point of inoculation a small abscess in the matter of which were several kinds of

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\* *Bacteria and their Products*, p. 286.



micro-organisms. One of these if inoculated into another animal produced identically the same symptoms.\*

*Q. These bacilli or their spores are very prevalent, I suppose?*

A. They are present in almost every sample of garden or field soil and have been found in the grime on a working man's hand and on dirty surgical instruments.

*Q. Then how is it that every wound, every abrasion of the skin when contaminated by dirt of any kind is not followed by this comparatively rare and deadly disease?*

A. It is because the bacilli

"are not endowed with high vitality, and hence do not invade living tissues unless these have been previously bruised or damaged by the presence of septic inflammation. They grow in the neighbourhood and near the surface of septic wounds, the septic organisms absorbing all the oxygen present and so originating the anaerobic [living without oxygen] conditions necessary for their development."†

*Q. What happens when the bacillus finds itself in favourable conditions?*

A. It is said to produce a general condition of poisoning which acts like strychnine on the spinal marrow and brain.

*Q. Has bacteriology done anything to cure this disease?*

A. Parietti, of Pavia, experimented on animals with a view to making them immune against tetanus infection by means of weakened cultures. Then from the vaccinated animals Behring and Kitasato derived a serum which they called anti-toxic and capable of preventing and curing tetanus. This when injected into the body is supposed to render the nervous system insensible to the poison. Vaillard, however, maintains that the anti-tetanus serum, while it has a curative effect on animals, has none on man.‡

*Q. What then is the value of the serum to a human being suffering from tetanus?*

A. Dr. Monti says:—

"The result, however, is favourable only when the injection is made within the first thirty-six hours of the disease. Up till now the clinic has not demonstrated sufficiently the efficacy of this remedy in the treatment of tetanus. Nevertheless preventive in-

\* *Bacteria and their Products*, p. 286.

† *Manual of Surgery*. Rose and Carless, p. 122.

‡ *The Fundamental Data of Pathology*. By Dr. Achille Monti. 1900. P. 246.

tions of the tetanus anti-toxin are indicated in cases of suspected wounds."\*

Q. *I see, it is very much like Pasteur's prophylactic for hydrophobia. If you have not contracted the disease you stand some chance of being cured of it; if you have it, it can do nothing for you?*

A. Messrs. Rose and Carless in their *Manual of Surgery* (p. 124) say:—

"At present the results of this treatment have proved disappointing, since few cases of acute tetanus have been saved by it, and the effect even in the more chronic cases is not at all certain."

Q. *What is the best means of preventing this poisoning of wounds?*

A. Absolute cleanliness, says Dr. Cabanès:—†

"Tetanus is a malady resulting from uncleanness. It is necessary then, when one finds himself in the presence of a wound, however small it may be—since even superficial wounds give rise to tetanus—not to abandon it to itself, but to close it."

Q. *Do you say that no cases have been cured by the serum?*

A. M. Berger, Surgeon at the Academy of Medicine, Paris, said:—‡

"In man, some cases of insidious tetanus have been, according to Tizzoni and Cantani, cured by the serum, but it is probable that these cases would have got well without any special treatment."

Q. *Is not the injection sometimes made into the brain itself?*

A. It is a most perilous proceeding, and the cases so treated usually die. The *Medical Annual* for 1900 (p. 576), records a case so treated where the patient recovered but died eight weeks after the last injection into the brain, and the results of the *post-mortem* leave but little doubt that the fatal issue was directly due to the injection. Carless§ has found that out of twenty-five patients treated in this manner eleven recovered and fourteen died.

Q. *Has tetanus been treated by injections of carbolic acid?*

A. Yes, and it is claimed to give better results than the anti-toxin treatment.||

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\* *The Fundamental Data of Pathology.* By Dr. Achille Monti. 1900. P. 247.

† *Journal de Médecine de Paris*, August 13th, 1893, p. 395.

‡ *Medical Press and Circular*, Paris Correspondence, June 7th, 1893.

§ *Practitioner*, July, 1899, p. 80.

|| See *Forty Years in the Medical Profession.* By Dr. Black, p. 168.

Q. *What is the name of that disease in cattle which causes Woolsorter's disease in men who work as hide-dressers, butchers, rag-pickers, and in kindred trades?*

A. It is called Anthrax and sometimes splenic fever and charbon.

Q. *Is it caused by a bacillus?*

A. An organism called the bacillus anthracis is always associated with it, and Pasteur placed in commerce two vaccines as preventive inoculations for the anthrax of animals.

Q. *This disease peculiar to sheep and cattle is not I suppose cured by the inoculations, but they are used as a preventive?*

A. It is proved that M. Pasteur's vaccines "are of extremely uncertain strength and of equally uncertain protective value as against virulent inoculation. Also they are far from being free from risk—a very considerable percentage of the animals dying from the vaccinations. The Hungarian Commission recommended its Government to prohibit the use of M. Pasteur's vaccines; and the German and English observers viewed them with no greater favour. Nearly all observers agree that M. Pasteur's theories regarding the etiology of the disease, and the cause of the attenuation of the virus, are completely erroneous."

Q. *Has our Local Government Board said anything about Anthrax Vaccines?*

A. Dr. Klein, the medical authority on such matters to the Local Government Board, and Lecturer on Physiology, St. Bartholomew's Hospital, said,† regarding the protective power of inoculation:—

"Is a cultivation in which in course of time the *bacillus anthracis*, at first forming a copious growth, degenerates, and in which no spores had been formed, and further which cultivation loses, as we know, its power to infect with virulent anthrax, animals when inoculated; that is to say such a cultivation as M. Pasteur's vaccine professes to be: 'is such a cultivation, I say, perfectly ineffective too, in giving the animals some sort of immunity against fuller inoculation with virulent material?' The answer is simply, 'Yes; it is perfectly ineffective.'"

Q. *Then I presume that our Government does not advise its use?*

A. The Board of Agriculture has issued a leaflet on Anthrax "A 1—95"  
1. On p. 3 we read:—

"Inoculation on the system recommended by M. Pasteur, could not be adopted except by an expert accustomed to operate, but the

\* *The Protective Value of Anthrax Vaccination.* By Dr. J. H. Clarke. London, 1886, pp. 3-4.

† *Supplement to the Twelfth Annual Report of the Local Government Board, 1882-3, p. 208.*



results of the operation in this country and elsewhere have not been of such a nature as would warrant the Board in recommending it to stock owners as a means of dealing with outbreaks of anthrax."

Q. *Has Professor Koch expressed any opinion upon it?*

A. Certainly, and a strong one against it. In his reply to M. Pasteur, p. 35\*, he said:—

"The preventive vaccination of Pasteur cannot be considered practically utilisable, on account of the insufficient preservation that it gives against natural infection, on account of the short duration of that preservation, and on account of the danger to which it subjects men and non-vaccinated animals."

Q. *What do they think of it in Paris?*

A. Dr. Lutaud, Chief Editor of the *Journal of Medicine*, Paris, said†:—

"The statistical record of the mortality that has followed the treatment by anthrax vaccine is conclusive. Only a few among the thousands of facts available as evidence can be cited. In a farm in the environs of Laon, a flock attacked with anthrax was vaccinated as many as three times, at intervals of fifteen days without eradicating the disease. In a neighbouring farm horses suffering from no disease were vaccinated, and three perished as the result of the operation. The proprietor, M. Magnier, demanded the value of his horses, which was reimbursed to him. In the environs of Meaux, a veterinary surgeon having killed four cows with the famous vaccine, M. Pasteur paid for these animals, to cut short the abuse of the interested parties."

I have many other reports of very serious losses of horses, cows and sheep in consequence of these vaccinations, but the above must suffice.

Q. *Mr. Paget in a foot-note on p. 75 of his book, "Experiments on Animals," says: "The initial failures; here and there, in 1882-84, are still paraded as evidence against experiments on animals." See the "Zoophilist," February and July, 1893, and Bryan, "Anti-vivisection Evidences," 1895. What do you say to this?*

A. The *Zoophilist* and the *Anti-Vivisection Evidences* simply stated well-authenticated facts and the opinions of experts in bacteriology. In a well-known text-book, *Bacteria and their Products*, by Dr. Sims Woodhead, the reasons for the failures with anthrax vaccine are suggested as follows (p. 374):—

"Equally good results were not always obtained by the experimenters, but in some cases, at any rate, the experiments appear to have failed through want of attention to detail rather than from any

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\* *L'Inoculation Preventive du Charbon.*

† *Etudes sur la Rage.*

defect in the method itself, and from the failure to recognize that the initial virus is not always of the same strength, that different animals have very different degrees of susceptibility and natural immunity, and that the quantity of the virus injected very materially alters the conditions of the experiments."

All these factors are as likely to operate now as they were in 1882-84, and taken together they must always be sources of danger in the use of the vaccine.

*Q. Do cattle ever recover from anthrax under ordinary treatment?*

A. They do. Dr. Sims Woodhead (*loc. cit.*) says: "It must be borne in mind, however, that cattle very frequently recover from anthrax under ordinary treatment."

*Q. It is not surprising that stock owners should be chary of risking the dangers of this vaccine.*

A. But all the same bacteriologists interested in the trade of these inoculations and vaccines continue to advertise their wares and make great parade of statistics in their favour.

## CHAPTER XVIII.

## SERUM THERAPY.

Q. *I suppose there is a good deal to be said in favour of diphtheria anti-toxin?*

A. It has certainly attained a great vogue in medical circles. The *Medical Annual* for 1900 put the case for and against the anti-toxin very fairly. Dr. Fenn said (p. 168):—

“The general opinion of the profession, as expressed in print, is overwhelmingly in favour of the efficacy of this mode of treatment. Kassowitz, however, strongly opposed by Paltauf, has emphatically declared against the use of anti-toxin. In this he is partly supported by Lennox Browne. Kassowitz affirms that it does not reduce mortality, increases the incidence of nephritis [inflammation of the kidneys] and post-diphtheritic paralysis, does not affect the temperature, and does not improve the chances of the patient after tracheotomy.”

This author quotes statistics in favour of his contention, and strikes at the root of the whole subject by denying that the causal relation between the Klebs-Loeffler bacilli to diphtheria has been proved.

Q. *What is the meaning of the reference to the Klebs-Loeffler bacillus?*

A. That is the name of the organism which is said by the bacteriologists to be the cause of diphtheria. It is, however, commonly found in healthy throats and is often not discoverable in the exudation taken from the throats of patients suffering from diphtheria. Dr. Hansemann, assistant to Professor Virchow said\*:—“Loeffler's bacillus not only appears in diphtheria cases, but also occurs in many healthy persons, and also in several kinds of slight disorders, such as catarrh of the *conjunctiva tunica*. It is not safe, therefore, to consider it as the cause of diphtheria.”

Kassowitz† says, “It is now generally admitted that the Loeffler bacillus is not the exciting cause of diphtheria.”

Q. *What is this anti-toxin, and how does it act?*

A. It consists of the serum from the blood of horses previously rendered incapable of taking the disease, or of a

\* In a paper read before the Medical Society of Berlin, November 29th, 1894.

† *Medical Press and Circular*, June 15th, 1898.

principle separated from it, and which is called anti-toxin. These horses are rendered immune by the injection of the virus of diphtheria, which is produced by the culture of the diphtheria bacilli in bulk, the liquid so prepared becomes sufficiently active in a month. The horses which we are cautioned rather alarmingly must not be suffering from glanders, are then injected with the filtered broth culture. After repeated injections, the blood serum is said to become highly anti-toxic; the horse does not show any signs of illness during this process.

*Q. How is it known that the serum has acquired this anti-toxic property?*

A. It is tested by injecting guinea-pigs with a dose of the serum, together with a dose of diphtheritic poison containing the active bacilli. When the serum from the horse is found to have reached the stage when these mixed injections do not lead to symptoms of diphtheria it is considered to be sufficiently powerful. The horse is then bled and the serum is bottled for sale.

*Q. What about the horse?*

A. The animal is found to be safe from infection from the diphtheria bacilli.

*Q. Does the anti-toxin as sold to the public contain nothing but this serum?*

A. They add carbolic acid to it before bottling, to preserve it from putrefaction.

*Q. Has the carbolic acid any effect on the patient?*

A. Many doctors declare that it is the carbolic acid that does the good, if any result from the injection. Professor Bacelli, Director of the Royal Medical Clinic of the University of Rome, the most distinguished medical authority in Italy, has employed plain injections of carbolic acid in place of tetanus anti-toxins. Many doctors in Italy, Germany, France, and Russia have followed his example. The *Medical Brief* said (December, 1898):—

“When an aqueous solution of carbolic acid will produce identically the same therapeutic effects which follow the use of anti-toxin in indicated cases, and when the serum is never given *without* the antiseptic, then it is childish folly to deny that it is the drug rather than the serum which does the work.”

*Q. You say that anti-toxin is practically accepted by the medical profession as a cure for diphtheria?*

A. It is. I said it has a great vogue, but many very eminent authorities reject it as dangerous and useless. Thus



Dr. Hansemann, Virchow's assistant from whom I have already quoted, said\* :—

"The supposed effect of the serum in rendering human beings immune from diphtheria is not established. Numerous cases of diphtheria have occurred notwithstanding the injections of the pretended immunizing serum. It is not a specific or a certain cure. Even when injected on the first day of the attack, and in apparently slight cases, some children have died after the injections. The statistics of the serum treatment are less favourable than that of some other modes of treatment.

"The serum remedy is not harmless, but produces dangerous skin diseases and sometimes fatal kidney inflammation."

*Q. How do you account for the statistics frequently brought forward to show the reduction of the death-rate from diphtheria by the use of the anti-toxin?*

A. In the first place I do not say that the injections are valueless, but I believe the reduced mortality to be largely due to the much greater attention paid to the early treatment of throat diseases of late years, the use of antiseptics like carbolic acid, iodine and sulphur to the throat, and internal remedies such as perchloride of iron. All these remedies are used in conjunction with the anti-toxin, and many physicians are quite as successful in treating their cases with these remedies without anti-toxin as others are with its use.

*Q. Is there a microbe of pneumonia?*

A. There is, and some very curious facts are connected with it. The organism supposed to cause pneumonia is named pneumococcus. It infects man alone, and has not yet been found as a parasite of the lower animals.†

It has rarely been found outside the human body, but there it is a fairly common parasite, and not necessarily in persons suffering from any illness whatever. It was first discovered at the Pasteur Institute accidentally on inoculating rabbits with the saliva of a child who had died from hydrophobia. In 1881 Sternberg on inoculating rabbits with some of his own healthy saliva also discovered it. It has since been found as a parasite infecting the mouth in perfectly healthy persons. Sternberg found it in 20 per cent., and Netter in 15 per cent. of all cases examined.‡

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\* In a paper read before the Medical Society of Berlin, November 29th, 1894.

† *British Medical Journal*, September 21st, 1901, p. 760.

‡ *Ibid.*

Q. Surely this is a powerful argument against the germ theory of the origin of this and other diseases?

A. One would think so but the bacteriologist says the microbe of pneumonia is normal in man, and is only occasionally productive of the disease. The bacillus coli communis, associated with cholera and diarrhœa, is considered to be universal and even a useful parasite of the intestine, and is only actively pathogenic, *i.e.*, productive of disease when it finds the opportunity of penetrating below the mucous membrane.

Q. This seems to do away with the utility of what is called the biological test or the method of diagnosing a patient's malady by isolating the germ supposed to cause it and inoculating animals with it?

A. Yes, and Dr. Foulerton says\* in testing the micrococcus of pneumonia "as obtained from healthy saliva, its virulence for the rabbit would usually be found quite as well marked as the virulence of a strain isolated from, for instance, a case of pneumonia."†

Q. But I read recently in a medical paper, and also in a London Daily, articles to the effect that in obscure lung complaints which sometimes defy diagnosis it is necessary to take some of the patients' sputum and inoculate mice or guinea-pigs, and the verdict of the event gives the clue to the necessary treatment?‡

A. I was very glad to read these statements as I had the opportunity to expose the fallacy of the biological test in both journals. I pointed out that it might grievously mislead a doctor anxious to make a diagnosis in a difficult lung case if he took sputum from a patient and inoculated animals with it, and they died of pneumonia. This would, if he relied on the test, lead him to diagnose pneumonia where none existed and overlook, say tuberculosis, from which his patient really suffered.

Q. Is there a serum against pneumonia?

A. There have been experiments with a view to produce one but "no satisfactory progress has been made in establishing a rational serum therapy for the disease in man."§

\* *British Medical Journal*, September 21st, 1901, p. 760.

† *Ibid.*

‡ *Medical Press*, September 4th, 1901, and *Daily Chronicle*, September 6th, 1901.

§ *Osler's Principles and Practice of Medicine*, 2 ed., p. 548.



Q. What is your opinion about inoculation against cholera?

A. There are experts who say that the *comma bacillus* may or may not be found in cases of cholera. There is much diversity of opinion as to whether any kind of bacillus is the cause of the disease. In Calcutta, one of the Health Officers medical colleagues, Brigade-Surgeon Lieut.-Colonel Saunders opposed inoculation on the ground that it is futile as well as dangerous to health.\*

"In some cases the deaths among the inoculated have been eleven times greater than among the non-inoculated, and in others, there were no cases of cholera among the inoculated. The whole matter is still *sub judice* and in the experimental stage, and no one acknowledges this more freely than Dr. Haffkine himself."†

Q. What has been the effect of Haffkine's inoculations in the treatment of the plague?

A. The *Practitioner* published in October, 1900, a "Special Plague Number." Dr. Tanner Hewlett, the writer of the article, "Plague: Its Bacteriology," said:—

"I have spoken to many Indian medical officers on the subject [the treatment of the disease] and all, I think, without exception, speak of the serum treatment as valueless." (P. 408.)

The *Medical Press* (March 14th, 1900), in an article on the Report of the Anti-Plague Inoculation Commission, said that:—

"The Berar result is encouraging for the future of this particular branch of serum therapy, although its failure to establish a series of definite scientific conclusions must come as a disappointment to the enthusiasts of preventive medicine. The difficulties of this task have been simply enormous, and the Report practically amounts to an admission on the part of Mr. Haffkine that he has failed to obtain sufficient data to place his prophylactic serum among the established truths of medicine."

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\* *Standard*, August 25th, 1896.

† *Report of Indian Medical Congress in "Daily Graphic,"* January 24th, 1895.

## CHAPTER XIX.

### THE BIOLOGICAL TEST.

*Q. Tell me more about the Biological Test you referred to.*

A. What are called pathological laboratories are ever being started in connection with the great hospitals and also in 80 municipalities. Experiments on a great number of animals constantly take place with a view to discover more exactly what is the nature of the diseases from which patients suffer. For example, a man may have certain symptoms which point to lung disease. He may have a cough and this may cause expectoration. One doctor may diagnose his complaint as pneumonia, that is to say congestion of the lungs, in a chronic form, and another doctor may hold that his symptoms point to consumption.

*Q. Are the two diseases likely to be confounded ?*

A. Very frequently they are difficult to differentiate, in that case the assistance of the bacteriologist is invoked. Some of the patients' sputum (that is expectorated matter) is inoculated under the skin of guinea-pigs or mice. If the animals die an investigation on bacteriological lines takes place to ascertain the nature of the microbe which killed them. It is maintained by some physicians that it is only by such a method that difficult questions in diagnosis in lung and other diseases can be settled.\*

*Q. What do you say to that ?*

A. It appears very scientific at first sight and is well calculated to captivate the ignorant or the over-scientific mind. The bacteriologist, it must be remembered, is not necessarily a practising physician; his work lies in his laboratory, not at the bed-side, and he is by no means to be implicitly trusted in matters of diagnosis. It is well known that many species of pathogenic or disease-causing micro-organisms flourish in the mouths and throats of the healthiest of us. In the saliva, nasal secretions, or throat mucus of people suffering from no serious disease a powerful microscope will often reveal the germs of pneumonia, tuberculosis, diphtheria, &c. If such secretions or sputa are

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\* See *Medical Press*, September 4th, 1901.

sent to the bacteriologist, and he finds, say, the bacilli of tuberculosis, he writes to the doctor and tells him what he has found, it is obvious that the medical man, if too prone to place implicit confidence in the bacteriological methods of diagnosis, may be seriously misled.

Q. *Do you know of any such cases in your own experience?*

A. I do. A relative of my own was recently the subject of a chest complaint, the character of which was differently diagnosed by several physicians. One of them thinking to decide the question sent a sample of his sputum to a bacteriologist, who discovered therein the tubercle bacillus, and some guinea-pigs who were duly inoculated with it died, it was declared, of tuberculosis. This was held to be conclusive. The patient was extremely ill and his case was considered hopeless. He was, however, removed to the South Coast, and a physician was called in, who discarded the idea of tuberculosis and ignored the experiments on the guinea-pigs. He treated the patient for chronic pneumonia, and in two months the invalid was well and has remained so ever since.

Q. *Why did not the bacteriologists find the bacillus of pneumonia?*

A. The bacillus of pneumonia (Friedlander), says Dr. Sims Woodhead, "sometimes gives rise to pneumonia in mice, guinea-pigs, and dogs, but does not affect rabbits."\* Then there is the diplococcus of pneumonia (Fraenkel). This, says Dr. Osler,† is a wide-spread organism, at times present in the mouth secretions of healthy persons. Animals inoculated with such secretions might die and lead one to suppose that the healthy persons who entertained the bacilli were really the subjects of pneumonia. Dr. Hare says‡: "While the presence of tubercle bacilli gives positive evidence, their absence in a given sample of sputum is not negative evidence of an absolute character, for that particular specimen may be free from bacilli or they may have escaped the staining or the eye of the examiner."

Q. *What is meant by staining?*

A. Sections and other objects for the microscope are stained with aniline dyes which are absorbed by certain bacteria, microbes, and tissues, for which they may have affinity. Thus some micro-organisms take best a blue or violet stain, others a red stain.

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\* *Bacteria and Their Products*, p. 425.

† *The Principles and Practice of Medicine*, 2 ed., p. 547.

‡ *Practical Diagnosis*.

*Q. I suppose that in some secretions more than one species of disease germ may be found ?*

A. Yes. A patient might have the bacilli of tubercle in his lungs and in the secretions of the mouth, as Dr. Hare tells us, he may have the micro-organisms of pneumonia. He might have enough of them to kill the mice inoculated with the secretions yet not be suffering from the disease. He might at the same time be suffering from tuberculosis, yet the bacilli might escape the eye of the examiner. It is all confusion, and can but mislead the physician who trusts to such a method of arriving at a correct diagnosis.



## CHAPTER XX.

### INOCULATIONS AND OTHER SCANDALOUS EXPERIMENTS ON HUMAN BEINGS.

*Q. I have heard that experiments of a murderous character have been performed on human beings in connexion with bacteriological research. Can you tell me anything about them?*

A. I could fill a volume with them; they form a great blot in the escutcheon of medical science, and though undoubtedly practised by enthusiastic "researchers" are reprobated by the whole consensus of medical opinion. Yet from time to time the experimenters of the laboratory invade the hospital ward and commit offences on helpless patients that can only be rightly described by a very ugly term.

*Q. Will you explain?*

A. In 1891 Europe was scandalised by the revelations of cancer grafting in Paris made by Professor Cornil and by Dr. Eugen Leidig in Berlin against Dr. Hahn and Professor von Bergmann. Dr. Leidig declared that these surgeons had repeatedly experimented on patients in the hospital at Friedrichshain by inoculating them with cancer lymph. The doctors admitted the accuracy of Dr. Leidig's statement, but said that the patients so operated upon were past recovery. They also affirmed that it was necessary for them to select human beings for experiment, inasmuch as none of the lower animals would have been suitable for the purpose.\* Dr. Hahn reported the fact in a German medical paper.†

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\* *Pall Mall Gazette*, July 8th, 1891. *British Medical Journal*, July 25th, 1891, p. 214. See also its issue of August 29th, 1891, p. 495.

† *Deutsche Med. Wochenschrift*, 10th November, 1887, p. 987. The *British Medical Journal* gave the following translation of this report: "Herr E. Hahn believes that he has proved by experiment that cancer is transferable. He had removed particles of three nodules from a female patient suffering from *carcinome disseminé* with scissors, and had implanted them in different spots of the body. All three particles increased in size, developing in cancer."

*Q. What about the French experiments ?*

A. A poor woman was brought one day to a hospital in France suffering from cancer of the breast. An operation was performed with her consent. After the operation, and while she was still under the influence of the anæsthetic, the surgeon, Dr. Doyen, carefully inserted a bit of the cancer, which he had just removed, into the healthy breast of the patient. The wound healed, but in a few weeks she found a new cancer was developing in the opposite breast. Dr. Cornil read an account of these human vivisections before the Academy of Medicine in Paris at its meeting of June 23rd, 1891.

*Q. What was the effect of the disclosure on the members ?*

A. They were naturally horrified, and recorded their deepest condemnation. Dr. Leon Le Fort said : "In the name of French surgery and in the name of morality, I cannot too emphatically protest against this experiment." Dr. Moutard-Martin said : "It is an essentially criminal act." Dr. Laney said : "It is surgical immorality."\*

*Q. Have experiments in relation to tuberculosis been performed on human beings ?*

A. The *Practitioner*, for November, 1901, told the story with the strongest reprobation of the inoculation of an infant by his own medical father. The facts are the following, as described in the *Scandinavian Medical Archives*, 1879, Vol. XI., "The Danish physician, Dr. Lund, of the Isle of Samsø, communicated the particulars of an experiment on a human being. The milk was taken from a wretched, meagre cow, continually coughing, and suffering from inflammation of the lungs. The subject of the experiment was Dr. Lund's youngest son, a healthy, well-made boy. The baby was fed with milk from the said cow from February, 1865, to November of the same year; and from November, 1865, to August, 1867, the child was fed with the milk of another similar cow." The result of the experiment is described as follows : "When eight months old, scrofulous conjunctivitis appeared in the child, which still continued when Dr. Lund wrote his report. In Dr. Lund's opinion the disease could only be rationally accounted for by reference to the diseased milk."

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\* See *Bulletin de l'Académie de Médecine*, 1891, p. 906. "Sur les greffes et inoculations de cancer."



Q. *Have experiments in inoculating children with leprosy been made?*

A. August Hirsch\* speaks of the criminal experiment made by Bargigli in inoculating children, from six to eight years old, with the matter of a leprous tumour.

Q. *I have seen reports in some papers about experiments in inoculating patients with yellow fever virus. Can you give me the facts?*

A. Professor Sanarelli has recorded them fully in an Italian medical magazine now before me:† "Certain experiments on animals led him to suspect the existence of a very active specific poison. This poison is obtained by simply filtering the broth culture of *bacillus icteroides* 24 days old."‡ He determined to experiment on human beings with this deadly yellow fever poison which he had made in his laboratory. We have the results in his own words. I will give them first in the original. He says:—

"La febbre, le congestioni, le emorragia, il vomito, la steatosi del fegato, la cefalalgia, la rachialgia, la nefrite, l'anuria, l'uræmia, l'ittero, il delirio, il *collapsus*—infine, tutto quel complesso di elementi sintomatici ed anatomici, che nel loro apprezzamento combinato costituiscono la base indivisibile della diagnosi di febbre gialla, noi l'abbiamo visto svolgersi ai nostri occhi, dovuto alla potente influenza del veleno amarilligeno fabbricato nelle nostri culture artificiali."

Q. *What is that in English?*

A. "The fever, the congestion, the hæmorrhage, the vomit, the fatty degeneration of the liver, the headache, the pain in the spine, the inflammation of the kidneys, the anuria, the uræmic poisoning, the jaundice, the delirium, the *collapse*—in fine, all that complication of symptomatic and anatomical elements which constitute when combined the indivisible basis of the diagnosis of yellow fever, I have seen unrolled before our eyes, thanks to the potent influence of the yellow fever poison in our artificial culture."

Q. *Where were these horrible experiments carried out?*

A. They were performed on healthy human beings in the quarantine station on the Island of Flores, near Monte Video.§

\* *Handbuch der Historisch Geographischen Pathologie*, 2nd Abtheilung, 1883, p. 32.

† *Annali d'Igiene Sperimentale*, 1897, Vol. vii., p. 470.

‡ *British Medical Journal*, July 3rd, 1897.

§ *Boston Evening Transcript*, September 25th, 1897.

*Q. Give me the exact details of these horrible experiments of Professor Sanarelli, if you have an authentic report of them?*

A. I have Dr. Sanarelli's own account of them, which he published in the *Annali d' Igiene Sperimentale* (Vol. VII., p. 441, *et seq.*). He says:—

"I have made experiments on five men. For reasons easy to understand, I have not made use of living cultures, but of broth cultures 15-20 days old, passed through a Chamberland filter, and for the sake of greater precautions, I have sterilized them by drops of formic aldehyde. On two men I tried subcutaneous injections, on the other three, the injections were intravenous. I sum up the following from the journal of my observations."

*Q. That is to say, I presume some of the inoculations were under the skin and the others directly into the veins?*

A. Exactly. The inoculations under the skin did not produce nearly such violent symptoms as did those into the veins.

*Q. Give me some details of these latter.*

A. In the case of E. N., aged 20, a Spaniard, we learn that soon after the injections he became violently sick, he rejected all the milk which he had drunk.

"At the same time, the patient is seized with a general disturbance in all his limbs; there is a violent and persistent pain in the lumbar region, which causes the patient to utter cries of distress and deprives him of even a single moment of rest."

*Q. Surely this was a most cruel experiment?*

A. This was but the beginning of the torment. The experimenter continues:—

"Little by little the abdominal region also becomes painful. The slightest application of the hands on those parts hurts the patient to an intolerable degree. In the meantime the axillary temperature goes up without interruption. . . . At about midnight the febrile reaction ceases, and the next morning the temperature is almost normal. . . . But the patient feels very unwell, and during the night not only has been sleepless on account of his lumbar and abdominal pains, to which a violent headache was added, but he has been also continually tormented by an irrepressible vomiting."

*Q. How long did these torments continue?*

A. For several days. The next day, November 4th, we read that:—

"The disturbance of all parts of the body becomes more intense; the patient complains of an inexpressible feeling of anxiety, which deprives him of any rest whatever, whilst shooting pains afflict the lumbar region with a distressing persistence."

Q. *Was the man delirious at night?*

A. Yes, we are told that:—

"The man tries several times to throw himself out of the bed, and the pain in the lumbar region torments him so much that he utters continual shouts of anguish."

Q. *Did this delirium last long?*

A. On the following day the same symptoms continued, and the pain became "still more distressing:—"

"There is continual vomiting, although the patient after the injection of toxin has not been able to take any food whatever."

Q. *Were any fresh torments invented for the poor creature?*

A. Oh, yes. On November 5th. Prof. Sanarelli tells us:—

"By means of sterilised pipes I make some exploratory perforations down to the liver and the kidneys, and with the mouth I draw from those parts some drops of fluid of which I avail myself to make cultures and microscopical researches."

Q. *What was the result of this fiendish work?*

A. The researcher says:—

"During the following night the patient improves to some extent, and after some days he recovers, but I discontinue my observations on him in order to give all my attention to what follows."

Then he gives an analytical and microscopical account of the patient's blood.

Q. *Were there any other victims to this atrocious experimentation?*

A. We have the full history of the other similar cases. The fourth experiment was made on N. Q., aged 35, a Spaniard. He was inoculated at noon on November 12th with the terrible virus and shortly afterwards the same dreadful symptoms began to manifest themselves. He suffered from persistent vomiting, and violent headache, great distress and pain all over the body. At eight o'clock in the evening we read:—

"Just then the patient is seized with a violent and restless fit of anguish, which gives rise to incessant lamentations. . . . His eyes have become red, moist, and bright, his pupils dilated, his look anxious and pathetic, and all this gives to the patient the appearance of a drunken man."

Q. *What was the result in this case?*

A. At five p.m. the patient became worse:—

"Pains more acute than before are felt in addition to frequent paroxysms of delirium. He can scarcely answer our questions and

insists on pointing out his forehead, his stomach, and his back as the most painful parts of his body."

Later on his "whole body is violently shaken by a fit of tremor."

"During the night the condition of the man has become worse." Vomiting and diarrhoea were almost incessant. At daybreak we learn that the patient is "completely prostrated."

*Q. Did he die?*

A. No, he lived for further experimentation. The researcher says:—

"I make aseptically a blood-letting of about 30 c.c.m. of blood which is allowed to coagulate in sterilized vessels in order to obtain its serum. I make also, as in the preceding experiment, exploratory perforations, taking care that the operations be quite aseptic, and I extract from the liver and the kidneys a small quantity of liquid which I hasten to scatter over various mediums in order to proceed to some microscopic investigations. At 8 p.m. begins to show some collapse."

*Q. What was the end of all this?*

A. "After some days he recovers."

*Q. There was still another case?*

A. Yes, but though the man was made very ill by the inoculation the symptoms were less intense than in the preceding cases, and the patient "entirely recovered."

*Q. Was the experimenter satisfied with his work?*

A. He says:—

"To show the meaning and the importance of the experiments which I have had the lucky chance of making upon human beings, it is not necessary to display any arguments. Whoever has observed personally some cases of yellow fever, or has acquired a knowledge of its symptoms by reading the text-books on the matter, will find that the experiments 3 and 4 represent exactly the typical cases of a severe attack of yellow fever,"

and he concludes that he has thereby demonstrated the existence of a microbic poison in yellow fever.

*Q. Yes, but has he found a cure for it?*

A. No. The best text-book of medical treatment of the most recent date says:—\*

"Attempts have recently been made to apply serum-therapy to the treatment of yellow fever, but the results have been most unsatisfactory; the serum has failed in the laboratory and at the bedside."

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\* Whittall's *Dictionary of Treatment* (p. 1020), London, 1902.



## CHAPTER XXI.

## SNAKE-BITE.

*Q. I have heard that one of the most brilliant results of serum-therapy is the treatment of snake-bite by the method discovered through experiments on animals by Calmette and Fraser. What do you say about it?*

A. Sir Joseph Fayrer in 1871 and again in 1884 published reports of many researches on Snake Venom, with a view to discover an antidote, he said\* :—

“To conceive of an antidote, as that term is usually understood, we must imagine a substance so subtle as to follow, overtake and neutralise the venom in the blood, and that shall have the power of counteracting or neutralising the poisonous or deadly influence it has exerted on the vital force. Such a substance has still to be found, nor does our experience of drugs give hopeful anticipations that we shall find it.”

*Q. Had many experiments on animals been performed before the researches of Fayrer and Calmette were made known?*

A. Very many, and the *Lancet*† said :—

“To explain the causes of failures in the experiments on animals is, in view of the successes on man, scarcely called for. With regard to dogs and cats, it is very evident that their motor nerve centres re-act very differently from those of man to both snake poison and strychnine. They show greater resistance to the former, for convulsions are not unfrequent at a stage when, in man, paralysis is all but complete.”

*Q. You mention strychnine, has that been used as an antidote to snake poisoning?*

A. Yes, and independently of experiments on animals. Ray injected strychnine solution in frequent doses, and the patient recovered.‡ Mueller strongly recommended its use as an antidote,§ and a cure in a case where Calmette's antivenine

\* “Address to Medical Society of London.” *British Medical Journal*, February 2nd, 1884.

† October 4th, 1891, pp. 959-60.

‡ *Medical Annual*, 1892, p. 445.

§ *Ibid.*, 1893, p. 466.

was not available is reported in the *British Medical Journal*, January 25th, 1902, p. 201.

Q. *What is the vivisector's remedy?*

A. It is called antivenine. Professor Fraser, F.R.S., made many experiments on cats, guinea-pigs, and rabbits, and in a lecture delivered at the Royal Institution in (I think) 1896, claimed that such animals when inoculated with gradually increasing doses of snake poison, acquire immunity, and that their blood-serum ultimately becomes a powerful anti-venine which gives perfect protection against the fatal effects of snake poison. He added that the antivenine was equally efficacious when swallowed.

Q. *That surely is a discovery attributable to experiments on animals?*

A. Not altogether. The Professor told his audience that he had read in the *Lancet* a communication from a doctor in South Africa describing the practice of swallowing snake venom followed by Kaffirs, and stating that these natives appeared to be rarely affected by snake-bites.

Q. *That is very interesting. Is the serum from animals poisoned by the venom of any species of snake equally an antidote?*

A. Dr. Fraser, of Edinburgh, claimed that the serum produced through cobra venom injected into animals made them proof against the bites of all snakes. But this was challenged by other experimenters. More experiments were made by Dr. Kanthack under the auspices of the Local Government Board.\* Dr. Kanthack came to the conclusion that cobra venom serum is not effective against the bites of all snakes.

Q. *Of course that would lead to more and more experiments.*

A. Yes. Whenever such a claim is made by one experimenter, another does his best by further experiments to upset it. There are great difficulties in the treatment of snake-bite for there are two venoms in the bite to be dealt with, one which affects locally and another which causes general intoxication. These two can be separated chemically and physiologically and evidently require different antidotes.†

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\* See Twenty-fifth Annual Report of the *Local Government Board*, 1895-96 (Medical Officers' Supplements).

† *Medical Annual*, 1898, p. 495.



Q. *What do practical surgeons say about it?*

A. One of our best Manuals of Surgery for Students and Practitioners\* says: "Probably the introduction of Fraser's antivenine will be the means of saving life." Dr. Whitla† says: "There are obviously enormous difficulties in the way of preparing the antidote serum or anti-venom." He recommends its employment and adds: "Calmette, however, urges the importance of thorough ligaturing of the limb, and irrigating the wound freely with a fresh solution of good Bleaching Powder in addition to the injection." Now the Chinese immediately adopt the ligature when bitten by a snake and Mr. Cantlie, F.R.C.S.,‡ says it "certainly is a fact, that but few, very few, Chinese die of snake-bite."

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\* Rose and Carless, *Manual of Surgery*, 1899, p. 199.

† *Dictionary of Treatment*, p. 878.

‡ *Medical Annual*, 1898, p. 495.

## CHAPTER XXII.

### BLOOD POISONING OR PYÆMIA.

Q. *What are the consequences of blood-poisoning ?*

A. The formation of abscesses, boils, &c. It is generally the result of some wound or injury by which septic material has found its way into the blood-current. It is called septicæmia and the bacteriologists say it is due to minute forms of vegetable life called *micrococci*.

Q. *Has every kind of septicism its special germ or micrococcus ?*

A. Dr. George Wilson\* has answered this question so ably that I cannot do better than quote his words. He says of Pyæmia, Septicæmia, &c., that—

“ Professor Ogston, of Aberdeen, Garré, Fränkel, and others have endeavoured to differentiate these several varieties of septicism by the appearances and the behaviour of the micro-organisms which abound in all of them, but the results are by no means conclusive. The so-called *streptococcus pyogenes* resembles closely the streptococcus of erysipelas, and the micro-organism associated with puerperal fever. Carbuncle, whitlow, and osteo-myelitis have been determined by Garré to be due to *staphylococcus pyogenes aureus*. Pyæmia, again has been divided into two kinds, presumably based on its microbial relations.”

Q. *Explain what he means by this ?*

A. Divesting the matter as far as possible from technicalities, I may say that by microbial relations in this connexion is meant that in one form of blood-poisoning the symptoms are supposed to be due to the absorption of a poison or poisons produced by germs not in themselves disease causing, that is to say, non-pathogenic organisms, and these symptoms constitute blood-poisoning. In the other form of pyæmia there is an open wound from which a constant supply of poisonous material enters the blood, and this is known as septicæmia or the micrococcus poisoning.

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\* *A Handbook of Hygiene and Sanitary Science*, 8th Edition. 1898. P. 499.

Q. Does Dr. Wilson accept this theory?

A. He says:—

"Unfortunately, however, for the strictly microbial origin of these septic diseases, Fränkel maintains that he has discovered the *streptococcus pyogenes* in the blood without giving rise to any symptoms."

Q. What do you think of this?

A. I think the dung-hill generates the fungi and not the fungi the dung-hill. Dr. Wilson adds that:—

"The tangled micro-biology of these septic diseases only serves to show that, given filth or filth infection, and septic microbes crop up in abundance; secure perfect cleanliness and wholesome conditions, and they are deprived of their power for microbic mischief."

Q. Surely every doctor, every one interested in sanitary science agrees with these wise words?

A. Yes, but the bacteriologist is trying to make our doctors believe that the way to cure these filth diseases is to inoculate the blood with more filth, to "cast out devils by Beelzebub."

Q. What! do they attempt to cure blood-poisoning by poisoning the blood?

A. Certainly they do. Mr. Stephen Paget in his *Experiments on Animals*, says, (p. 71), in his chapter on "Suppuration and Blood Poisoning"—

"Several cases have lately been reported, where recovery from this most hopeless disease has followed the use of the anti-streptococcal serum. It has also given excellent results in cases of puerperal fever, and in cases of dissection wounds. Already, though it is a new thing it has saved many lives, and is steadily gaining ground in practice."

Q. Is that really so?

A. The claims made for this serum are far in excess of the results obtained in practice. In a paper read by Dr. Herman (who is a great authority on the subject) at the Obstetrical Society of London, on November 1st, 1899,\* he said:—

"In some cases published as examples of the success from the serum, it had seemed to the author, from reading the reports, that some of the patients were never in danger, and that in some recovery was to be attributed to other therapeutic measures . . . used at the same time as the serum. For these reasons it seemed to him impossible at present to draw conclusions as to the value of the serum from statistical tables."

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\* *British Medical Journal*, November 11th, 1899.

Dr. Herman's paper gave rise to a discussion. Dr. Ewen Maclean thought that one good result would be the checking of the indiscriminate use of the serum. Dr. Herbert Spencer said that the results of his extensive inquiry were unfavourable to the treatment, and were corroborated by the investigations of the American Gynæcological Society. It was not free from danger, several deaths having occurred from the injections.

*Q. Have any of the recently published works on the treatment of diseases said anything on the subject?*

A. One of the best and most popular of these, the *Dictionary of Treatment*, is by Dr. Whitla, who is Professor of Materia Medica and Therapeutics in Queen's College, Belfast, and Senior Physician to and Lecturer on Clinical Medicine, Royal Victoria Hospital, &c., &c. The author says, p. 827 (4th Ed., 1902), under Pyæmia:—

"Anti-streptococcus serum has proved very disappointing, and though some cases seem to improve under its influence, it is clear that the serum-therapy of septic poisoning is still on a very different footing from that of diphtheria."

Again, under the heading "Puerperal Fever," p. 821, he says:—

"Attempts have been made to treat puerperal fever with serum prepared from animals vaccinated against streptococci. The results are certainly up to the present contradictory and unsatisfactory, though in this direction lies the hope of future progress."

*Q. I see that you have very little faith in serum-therapy for any disease?*

A. To present a cheque on the serum bank is in my experience to have it returned marked "no effects."

## CHAPTER XXIII.

## OVIARTOTOMY, ABDOMINAL SURGERY AND VIVISECTION.

*Q. I have heard that almost all our success in Modern Surgery is due to Vivisection, for example, in Abdominal operations such as Ovariectomy.*

*A. Yes.* The Bishop of Peterborough said in the House of Lords, July 15th, 1879, that, "A London Medical man of the highest eminence [Sir Spencer Wells] owes a discovery, by which he has saved hundreds of lives, to a series of experiments performed upon a dozen rabbits." He referred to the operation known as Ovariectomy for the successful performance of which Sir Spencer Wells was justly renowned. But the distinguished surgeon had said in his book, *Diseases of the Ovaries*, London, 1872, p. 372 :—

"If we could hope in diseased women for the same series of changes as have been observed in healthy dogs and rabbits, we might agree more completely with the conclusions of the German experimenters. But it is one thing to remove a piece of a uterine horn, or a healthy ovary, or a bit of omentum or mesentery, from a dog or a rabbit, and a very different thing to remove a large uterine or ovarian tumour from a woman whose general health has been more or less affected by the growth of the tumour."

*Q. Did Sir Spencer Wells actually discover the operation in question?*

*A. No, he did not. He says himself\* :—*

"No one can dispute the validity of the direct claim of McDowell as practically the first successful ovariectomist. . . . He lost only the last of his first five cases of ovariectomy, and thus as it were established at the outset what until recently was complacently regarded as a satisfactory standard of mortality for so serious an operation."

*Q. This is certainly conclusive. When did McDowell die?*

*A. He studied under John Bell, in Edinburgh, in 1794, and died in 1814, aged 78.*

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\* *On Ovarian and Uterine Tumours, &c.* (1882), pp. 184-5.



*Q. What was the date of Sir Spencer Wells' first case of ovariectomy?*

A. He says, in the work from which I have just quoted:—

"During the autumn of 1857, a young woman was under treatment (at the Samaritan Hospital) for what appeared to be an ovarian tumour on the left side. . . . I determined to see what it was, and in December, 1857, twenty-four years ago, I prepared for my first ovariectomy. . . . As soon as I opened the peritoneum, and it was proved beyond all doubt that the tumour was behind the intestines, I was induced very unwillingly to close the wound, and do nothing more."\*

*Q. Did any other surgeon practice ovariectomy at this time?*

A. Certainly. Dr. Charles Clay, of Manchester, who was born 1801 and died 1893, has left on record his experiences in this connexion, and has protested in the strongest terms that he successfully performed the operation of ovariectomy and that without the slightest hint from experiments on animals. He said †:—

"Fifteen years after my first operation (in 1842) Mr. S. Wells came to Manchester to be present at one of my operations. . . . I was for some time after in correspondence with Mr. Wells but never heard of vivisection in connexion with ovariectomy, nor can I perceive any advantage that ovariectomy has received from such experiments. All my operations from first to last have shown the same average amount of success—about 75 per cent. I have never practised nor yet countenanced vivisection. I have given operating after 400 cases and about 100 deaths."

Again in a letter signed CHARLES CLAY, M.D.,† he said:—

"I have lived to see ovariectomy established as an operation years before Mr. S. Wells ever operated."

In another letter to the *British Medical Journal* (July 17th, 1880, p. 110), he said:—

"In my opinion vivisection has no more to do with advancing the success of ovariectomy than the Pope at Rome."

*Q. Was Dr. Clay's claim admitted by the heads of his profession?*

A. Certainly it was. Sir James Simpson, the discoverer of anaesthesia by chloroform, wrote to him in 1847 as follows:—

"My dear Clay,—The operation is your own; none can rob you of your claim. Call it ovariectomy, not peritoneal section. Your success is brilliant."

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\* *On Ovarian and Uterine Tumours, &c.*, by T. Spencer Wells (London, 1882), p. 196.

† Letter dated April 6th, 1880.

‡ *British Medical Journal*, July 3rd, 1880, p. 32.

Again, Mr. Lawson Tait, in a letter to the *British Medical Journal*, said with reference to the various periods in the history of the operation:—

"The second phase begins with Charles Clay, who first performed ovariectomy in England, on September 27th, 1842; and during the succeeding twenty-five years he performed 390 ovariectomies, with a mortality of very nearly 25 per cent."

*Q. Lawson Tait was himself a highly successful ovariectomist, was he not?*

A. He was the most successful operator in the abdominal region who ever practised. He says\*:—

"The whole progress of abdominal surgery dates from the first successful case of ovariectomy performed by Robert Houston in 1701. Failing to see the lesson taught by this, and led astray by vivisection, no further success was achieved till 1809, by Ephraim McDowell, and it was not till 1867 that any substantial gain was made. Disregarding all the conclusions of experiment, Baker Brown showed us how to bring our mortality of ovariectomy down to 10 per cent.; and again, in 1876, Keith proved that it might be still further reduced. The methods of this reduction were such as only experience on human patients could indicate; experiments on animals could and did teach nothing, for operations have been performed on thousands of animals every year for centuries, and nothing whatever has been learnt from this wholesale vivisection.

"As soon as Keith's results were established abdominal surgery advanced so rapidly that now, only six years after, there is not a single organ in the abdomen that has not had numerous operations performed upon it successfully. I have had, as is well known, some share in this advance, and I say, without hesitation, that I have been led astray again and again by the published results of experiments on animals, and I have had to discard them entirely."

*Q. Is abdominal surgery entirely of modern origin?*

A. Certainly not. For example: We know that 500 years B.C. there were surgeons in India who opened the abdomen for bowel obstructions. Amongst the legends of Gotama Buddha is the history of Jiwaka which illustrates the state of medicine and surgery in India at that early age. In Mr. Spencer Hardy's translation of Singhalese MSS.† we read that Jiwaka operated on a nobleman in Benares who had twisted a part of his intestines into a knot so that he could not pass any solid food.

"Crowds of physicians came to see him, but none of them dare undertake his case; but Jiwaka said at once he could cure

\* *The Uselessness of Vivisection*, p. 27.

† *A Manual of Buddhism*, p. 238.

him. He bound his patient to a pillar that he might not move, covered his face, and taking a sharp instrument, without the noble's being aware of what was going on, ripped open the abdomen, took out his intestines, undid the knot, and replaced them in a proper manner. He then rubbed ointment on the place, put the patient to bed, fed him on rice-gruel, and in three days he was as well as ever."

Q. *That hardly seems possible.*

A. No doubt, but fact or not, it at least shows that at that early period such operations were not considered impracticable, besides, the method of treatment differs very little from that of a Lawson Tait or a Treves, only their patients do not recover so rapidly.

Q. *Did the ancient Greek surgeons practice on the abdomen?*

A. No doubt they did, as we know that Herophilus and Erasistratus, great anatomists of the Alexandrian School, performed all sorts of surgical experiments on healthy human beings handed over to them for that purpose. The ancient apologists for these human vivisections used to say that "when the intestines protrude through a wound, a person who does not know what is their colour when in a healthy state cannot distinguish the sound from the diseased parts, nor therefore apply proper remedies; while, on the contrary, he who is acquainted with the natural state of the diseased parts will undertake the cure with confidence and certainty; and that in short, it is not to be called an act of cruelty, as some persons suppose it, to seek for the remedies of an immense number of *innocent* persons in the sufferings of a few *criminals*."\*

Q. *That last apology for human vivisection is just on the same lines as that made by our vivisectors now, "the sacrifice of a few rabbits and the immense gain to suffering humanity."*

A. Exactly, and I can tell you that our vivisectors are by no means ashamed to demand that criminals should be used for surgical experiment now. For instance, Dr. J. S. Pyle, of Canton, Ohio, has issued pamphlets in pursuance of his plea that condemned criminals should be given over to vivisectors for physiological experiment. At his request a Bill was actually introduced in the Ohio Legislature, November 3rd, 1894.†

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\* See Berdoe's *Origin and Growth of the Healing Art*, pp. 197-98.

† See the *Zoophilist*, Vol. XVI., p. 86.

*Q. Tell me more about the progress of Abdominal Surgery before modern times.*

A. In the days of the Vikings, says Dr. Puschmann,\* mention is made of the operation of gastroraphy or sewing up a wound of the belly or some of its contents. Albucasis, the skilful Arab physician, practised the stitching of the bowel with threads scraped from the internal coat.† Roger, of Palermo, who lived about 1210, and was a professor in Salerno, was a very famous surgeon, who practised stitching of the intestine. Then coming down to the 16th century we have the remarkable operations in abdominal surgery performed by Swiss sow-gelders. One of these performed the first Cæsarian operation on the living patient, the other performed the operation of ovariectomy successfully on his own daughter.‡

*Q. I perceive that Abdominal Surgery has not had much to do with Experiments on Animals?*

A. Oh, yes it has! but only to retard its progress. Let me give you the words of Sir Frederick Treves, one of the Royal Surgeons, who said in "An Address on Some Rudiments of Intestinal Surgery," delivered before the Midland Medical Society at Birmingham: §

"Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel that when I came to operate upon man, I found I was much hampered by my new experience, that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestine."

*Q. I have heard that a method of joining divided portions of the intestines by means of what is called "Murphy's Button" was discovered by vivisection. Was that so?*

A. Sir Frederick Treves tells us || that the idea did not originate with the surgeon whose name the button bears, but was conceived some seventy years ago in the brain of one Denans. There are any way "several grave objections to its employment as a routine measure," ¶ and fatal issues have occurred from its use.

\* *Hist. Med. Education*, p. 186.

† Baas, *Hist. Med.*, p. 231.

‡ Berdoe's *Origin and Growth of the Healing Art*, pp. 361-62.

§ Reported in the *British Medical Journal*, November 5th, 1898. *Ibid.*

¶ Rose and Carless, *Manual of Surgery*, p. 900.

Q. *Professor Lawson Tait I know was a very distinguished abdominal surgeon, has he said anything on the question in any other connexion than that of ovariectomy?*

A. He sent me the following letter a few years ago in reply to a question I asked him:—

“Birmingham, October 9th, 1889.

“DEAR DR. BERDOE,—You may take it from me that instead of vivisection having in any way advanced abdominal surgery, it has, on the contrary, had a uniform tendency to retard it. This I show to be particularly the case in operations upon the gall-bladder, and refer you to the current number of the *Edinburgh Medical Journal*, where, in an article, I point to the fact. As to the use of the anti-septics of Lister, it increased our mortality, prevented recoveries, and did a vast deal of harm by retarding true progress.

“Yours very truly,

“LAWSON TAIT.”



## CHAPTER XXIV.

### ABDOMINAL SURGERY—THE KIDNEY.

Q. *What is the operation called Nephrectomy?*

A. It is the total removal of the kidney, and is performed for disease or injuries of the organ, and can be undertaken through the abdomen or through the loin.

Q. *I read an article on "Recent Advances in Surgery" some years ago in Harper's Magazine, and some letters in the Bazaar, Exchange and Mart, in which it was claimed that amongst other triumphs of vivisection this operation for removal of the kidney had been discovered through experimentation on animals. Was that so?*

A. It was not, and I disproved the claim at the time. I am glad you have given me the opportunity of doing so once more. There is an operation of a similar kind called Lumbar Colotomy or the opening of the intestine in the loin, which has been practised since the year 1839, when it was made known to the surgical world by Amussat, who said he had learned it by experiments on the dead body. Thus easy and safe access to the kidney was provided by a method which owed nothing to vivisection and was in itself an important step. The next question for the surgeon to consider was the possibility of removing the organ without endangering the life of the patient. Could a man live and be in health with only one kidney? This had long been settled in the affirmative by the revelations of the *post-mortem* table.

Q. *Is that really the fact?*

A. Certainly it is. In the *Lancet*, December, 1889, p. 1270, Mr. Knowlsey Thornton published a lecture on "The Surgery of the Kidneys," wherein he refers to "the occasional absence of a second kidney, the knowledge that in one case at least (Polk's) a single kidney has been removed, the error only being discovered after the death of the patient."

Q. *This shows that vivisection had nothing to do with the demonstration that we can live with one kidney only?*

A. Just so. But those who are familiar with the writings of Hippocrates, the Greek surgeon who lived about 400 years before our era, know that the operation for removal of

a diseased or injured kidney was a common one in his time. Cardan laments it as "one of the lost operations." He quotes a writer as having met with a case where eighteen stones were removed from the loin of a patient.

*Q. Was the operation ever performed on condemned criminals as an experiment by the Italian surgeons who were very active in this direction?*

A. No doubt it was; for Italian surgeons practised it in 1696, and in 1680 it was performed on an archer of Meudon, who had stone in the kidney, and had been condemned to death for some crime, but the doctors got him handed over to them to experiment with to see if they could remove the stone without killing him. The operation was successful, and the man lived for several years.\*

*Q. Have you any records of more recent date?*

A. Yes. In 1860 a surgeon named Walcott, of Milwaukee, U.S.A.,† removed a cancerous kidney, thinking it was a tumour of the liver. Other operators did the same in various instances, yet as the patients lived without discomfort it was quite unnecessary to experiment on dogs to demonstrate a fact already so well-known.

*Q. Can you refer me to any great modern surgeon whose authority would settle this point?*

A. You can have no greater authority than that of Mr. Erichsen, who in his great work on surgery,‡ says:—

"Before proceeding to the removal of the kidney it was necessary to ascertain that a person could not only live, but that the health might be maintained after the removal of so important an organ. That this is possible has been proved by the result of inquiry, by pathological result, and by physiological experiment. There are cases on record in which, in consequence of a deep stab or cut in the loins, one kidney has been wounded and forced out of a wound whence it has been removed, the patient making a good recovery. Then, again, it has long been known to pathologists that a person may live with one kidney practically useless, either blocked by calculus or destroyed by suppuration."

*Q. That is conclusive, but I have heard of recent experiments on dogs by removing portions of healthy kidney from time to time, for what were such cruel operations performed?*

A. You refer to Dr. Rose Bradford's experiments on dogs' kidneys at the Brown Institution. By tying the tubes which

\* Mezian—*Abrégé Chronologique de l'Histoire du France.*

† *Phila. Med. and Surg. Rep.*, 1861.

‡ *Science and Art of Surgery*, Vol. II., p. 980.

lead from the kidney to the bladder he succeeded in distending the kidney to the size of the fist after causing suffering lasting from 10 to 15 days.\* Again we find his records of some particularly cruel experiments on "The Kidney and Metabolism,"† which consisted in removing large portions of the kidney from 49 living dogs. The object was to discover how the animals could sustain life and for how long, with portions of their kidneys cut out from time to time.

Q. *Were not these operations performed under anæsthetics?*

A. Yes, but note the details. "The animal was anæsthetised with chloroform, a hypodermic injection of morphia . . . was then given, and the chloroform administration was continued throughout the operative procedure." That is to say, while the actual cutting was in progress the animal was duly chloroformed. But when these incisions, &c., were finished the *period of observation* began where anæsthetics of course could not be given. Great emaciation, thirst, weakness, vomiting, loss of appetite, diarrhœa, ulceration, &c., are noted as preceding the deaths. One of the dogs died on the fourth day after the operation, another on the sixth, a third 36 days afterwards. Then 28 dogs were used for a second operation—the removal of the entire kidney on the opposite side. All these mutilations so far as surgery was concerned were of no practical value, as the whole surgery of the human kidney was mastered by the operations on men which I have described to you.

Q. *Is not the kidney a favourite organ for experiments?*

A. It is, and such experiments are usually of a very cruel character. Prominent amongst them were those employed by the late Professor C. S. Roy at Cambridge and described at length by him in a paper "On the Mechanism of the Renal Secretion."‡ The method of research then invented has been employed up to the present time.

Q. *Will you describe it?*

A. By means of an apparatus which Roy called the Oncometer the variations in the size of the kidney under different conditions in the living animal can be measured.§

\* See *Lancet*, December 11th, 1897.

† *Journal of Physiology*, Vol. XXIII., No. 6.

‡ *Proceedings of the Cambridge Philosophical Society*, May 23rd, 1881.

§ *A Text Book of Human Physiology*. Landois and Stirling, 3rd ed., p. 428.

Thus in poisoning by strychnine the kidney shrinks, and in a curarised animal when respiration is arrested a rapid and great diminution of the size of the kidney takes place. Other drugs have a different influence on the organ, and consequently a great number of experiments are constantly performed in this connexion.

*Q. How can the experiment be managed without killing the animal?*

A. The operation is the same as that for nephrotomy or exposing, exploring and incising the kidney for disease. The loin is opened and the kidney exposed, cleared of all its connexions, leaving only intact the vessels, &c., which enter its substance. It is then placed in the oncometer, which is a metal box which has been previously warmed, and has two compartments filled with warm oil. There is also a flexible tube attached to it connecting the box with a recording instrument.

*Q. Of course the animal is under complete anæsthesia?*

A. Dr. Roy says in his paper:—

"It need scarcely be added that the animal—rabbit, cat or dog, in most cases the latter—was kept fully under the influence of ether, chloroform or morphia, or a combination of two of these, from the commencement to the end of the experiment."

*Q. Well that is satisfactory—*

A. On the contrary, it is and must be absolutely untrue. Ether, chloroform, and morphia exercise a potent influence on the blood pressure, and so on the volume of the kidney, and it would be as useless and unscientific to attempt under such conditions to study the normal condition of an organ so delicate and sensitive as the kidney, as it would be to study the effects of brandy on a patient already intoxicated with whisky. The action of drugs on the kidney such as digitalis, nitrate of soda, and acetate of potash is described by Dr. Roy in his paper as having been tested by his new method. Digitalis first contracts and then expands the organ. Nitrate of soda and acetate of potash expand it, and so on. Even curare affects it, causing a contraction of the kidney vessels. We are told that "the kidney vessels are exceedingly elastic, a rise or fall of the aortic blood pressure causing an expansion or contraction of the kidney which may be very considerable in amount." I cannot therefore believe that chloroform, for



example, would be administered to an animal, the mechanism of whose Renal Secretion was being tested by Roy's method when we know that the blood pressure is usually lowered by the inhalation of chloroform, and if the dose is strong it may fall very considerably, and may even be reduced to zero.\*

*Q. I quite appreciate your point, and I understand that the drugs named would so mar the results of the research that it would be absurd to use them. I presume that such experiments performed without anæsthetics would be extremely painful?*

A. I can imagine few operations and experiments more cruel. Some of the worst experiments on animals have been performed in this connexion at Cambridge, and, as I happen to know, without anæsthetics, as in a public debate at that place they were admitted to have been inapplicable. The chest has been opened, the kidney dissected out by cutting through the loins, the skull opened, the spinal canal exposed, the nerves dissected out, tied, cut and stimulated with electricity. Then drugs were injected into the veins to test the effect on the kidney secretion.

*Q. With what result to practical medicine?*

A. The most misleading results. Here is an example. Brunton and Power found that digitalis diminished or entirely suppressed the secretion in the dog thus mangled. Yet in the human subject digitalis is the most reliable of all drugs for increasing the secretion, when from disease such as dropsy it is greatly diminished. Kidney disease has not been relieved by any vivisectional experiments whatever.

*Q. Do you not think that the assurance that true anæsthetics were efficiently used in these experiments probably applied only to the operation for exposing and arranging the kidney, and that when this was completed the animal was permitted to recover from the anæsthesia and then the period of observation began when the animal's normal condition was restored, if that were possible?*

A. Yes, no doubt that might be so. We read that, "After the immediate effect of the operation has passed off, the volume of the kidney will usually remain unchanged" (p. 4). It is noteworthy, too, that we read, "Arrest for three or four minutes of the artificial respiration where that is employed, and where curare has been previously injected, causes a contraction of the renal vessels," &c.

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\* *Pharmacology*, Lauder Brunton, p. 723 (1st ed.).



*Q. Really it would be a sufficient indictment against Vivisection in England under the present Act of Parliament to base your charges on these horribly cruel kidney experiments.*

**A.** Yes, I think the anæsthesia sham is well exposed in this connexion, and, that being so, the cruelty is sufficiently obvious.

## CHAPTER XXV.

### ABDOMINAL SURGERY—HERNIA.

*Q. Has the treatment of hernia or rupture been improved by experiments on animals ?*

A. On the contrary, it did not improve until surgeons abandoned the mistaken treatment into which they were led by Vivisection.

*Q. Tell me about that.*

A. Before the Royal Commission\* Mr. George Macilwain, F.R.C.S., was examined and asked his opinion as to the cure of infirmities, surgical and medical, and the relation of animal experimentation to that end. He replied: "I know of no error in the whole practice of surgery (and now I am obliged to mention myself, which is not a very pleasant thing) which has produced an evil equal to that which I myself have practically corrected, and that is the employment of purgatives after the operation in strangulated hernia."

He was asked, "Has that mistake resulted from experiments on living animals?" and he replied, "Yes; and now I wish to show you that."

*Q. Before you proceed pray explain to me what strangulated hernia really is ?*

A. I will give you Mr. Macilwain's own explanation to the Royal Commission. He said: "I must tell you that strangulated hernia was, and is now, a very dangerous disease. The intestine escapes from its natural cavity. It is constricted by the tendinous structure through which it escapes, and the consequence is that the case is a very uncertain one; I have seen a patient escape after days and I have seen mortification take place within an hour or two; so that it is a case of great gravity. The operation consists of cutting down to the part and enlarging the opening of the tendinous structure and replacing it. Now, of course, it is a very natural anxiety for a man to know whether the bowels have acted, because it

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\* *Blue Book*, pp. 96-97. Q. 1,850 *et seq.*

is a direct proof that the stricture has been removed, but that led to a most grievous mistake. And now I must go back to the experiments."

Q. Did not a Mr. Travers make experiments on dogs, and describe them in a book on *Hernia*?

A. Yes; his work is entitled, "*On Injuries of the Intestines and on Strangulated Hernia.*"

Q. What experiments did he make?

A. Mr. Macilwain described them to the Royal Commission: "He made some experiments on animals, and divided the intestines and sewed up the wound and guts, and did a great many things of that kind, and showed that animals have certainly very great powers of repair under such circumstances. But now the inductive philosophy comes in. He left out some of the most important parts of the subject as regarded the human subject, because he never purged these dogs. If he had wanted to carry the analogy clue for strangulated hernia he should have placed the dog (even supposing it was feasible to do anything with the dog at all) as nearly as possible under the same circumstances as the human subject."

Q. That I presume would not be practicable?

A. Of course not. A dog with healthy intestines is in a very different condition from a human patient with a bowel in which constriction had impeded the circulation and possibly set up mortification. But the experimenter ignoring this, "goes on to the treatment of strangulated hernia, and he says that after the operation the great thing is to get a discharge from the bowels (now this is true); and the great danger is from the inflammation of the peritoneum, that is the membrane covering the bowels, and lining the interior of the body; and he says that purgatives are the great thing; that if there is no peritonitis, we use purges to prevent it, and if there is peritonitis, then we use purges to cure it."

Q. How did that work in practice?

A. Mr. Macilwain says, "In the same book in which Mr. Travers published these experiments he also published the treatment of strangulated hernia; he gave his directions for the treatment which is exactly that which destroys the patient."

Q. Does Mr. Macilwain give any proofs of that?

A. He says (Q. 1,852) in answer to a question whether Mr. Travers's experiments upon animals in this connexion had

absolutely misled him: "But now here comes a very extraordinary circumstance. Here is the transcript of a lecture, the part I refer to being only a few lines, from a gentleman whose mistake induced me first to write upon that subject, and which has been the means of my having any power of claiming that improvement as my own. I could not get him to refrain from giving purgatives, and the patient died. That same gentleman, Mr. Stanley, of St. Bartholomew's Hospital, in lecturing to his pupils some 20 years afterwards, or more than that, says as follows: That at one time purgatives were employed in these cases, whereas it is now perfectly understood that they ought not to be so employed, and he had himself had bushels from cases of strangulated hernia—cases where the peritonitis was traceable to the purge."

Q. *Was Mr. Macilwain himself an experimenter on animals?*

A. He was to some extent, but he explained, he said (Q. 1,857): "I only repudiate vivisection as one of the fallacies in medical investigation."

Q. *What you have told me throws great light on the statement you recently quoted from Sir Frederick Treves in which the Royal Surgeon who saved the King's life said that his experiments on the intestines of dogs had hampered him when he came to operate upon man, that he had everything to unlearn, and that his experiments had done little but unfit him to deal with the human intestine.*

A. Mr. Macilwain's evidence is the best corroboration of that.

## CHAPTER XXVI.

### ABDOMINAL SURGERY—THE STOMACH.

*Q. Upon the stomach as the principal organ of digestion, of course, many experiments on animals have been made?*

A. Naturally, but think how absurdly. Your dog eats bones, and actually digests them, so that his evacuations are loaded with the lime salts of the bones. His gastric juices are of such a powerfully solvent character that they digest the hardest bone. What would our condition be after such a meal?

*Q. Describe some of the experiments on the stomachs of animals.*

A. The question has been discussed as to the share of the stomach in vomiting. Some physiologists have held that it is quite passive during the act of vomiting and that the expulsion of its contents is due to the pressure of the abdominal muscles and the diaphragm.\* To decide this Majendie substituted a bladder for the stomach in a living animal, but the abnormal and painful conditions in which the animals underwent the experiments must have destroyed the value of any such observations. With reference to these experiments Kirke says:† “They by no means show that in ordinary vomiting the stomach is passive, and, on the other hand, there are good reasons for believing the contrary.”

*Q. What is the scientific name for the removal of the stomach?*

A. It is called gastrectomy. A dog was exhibited at a meeting of the Paris Biological Society on November 25th, 1893, whose stomach had been removed five months previously. It was kept alive by administering small quantities of milk at frequent intervals. Drs. Baudry and Peabody reported in a letter published in the *Zoophilist*, September 1st, 1893, how they saw a case of the kind in a laboratory in Paris. “On opening the door we heard moans and cries of pain. On entering we saw a little half-bred poodle dog bound

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\* Kirke's “Physiology,” 8th Edition, p. 289.

† *Ut supra.*



to the table, mangled and bleeding, perfectly conscious and evidently in very great pain. Every now and again a low, long groan would escape from it. We asked what had been done to it? and were told by one of the assistants that its entire stomach had been removed at 10.30 that morning, the object being to see how long it would survive the loss of its stomach. No anæsthetics had been used."

*Q. Did the poor creature survive?*

A. The doctors tell us that they were informed that "it had died in the night some time." Other terrible operations went on in the same place without anæsthetics whilst the vivisectors were coolly smoking and chatting merrily. "To add to the horror of the scene a young lady was present, who looked on calmly."

*Q. I believe the stomach has been removed from human patients—for cancer or some other malady?*

A. That is so. Several cases have been recorded, and in cases of cancer relief may be afforded for a few months before the patients again go down hill.

Mr. Lawson Tait says:\* "There is not a single organ in the abdomen that has not had numerous operations performed upon it successfully. I have had, as is well known, some share in this advance, and I say, without hesitation, that I have been led astray again and again by the published results of experiments on animals, and I have had to discard them entirely."

*Q. This is just what Sir Frederick Treves said about Abdominal Surgery?*

A. Exactly, and as Sir Frederick is not an anti-vivisectionist, yet confirms Professor Tait, who was such, we may certainly conclude that by the testimony of two such eminent experts on different sides the case is settled in our favour.

*Q. Does Mr. Tait refer to Operations on the Stomach in cases of Cancer?*

A. He said:

"Speaking of some recent attempts which have been made to operate on cases of cancer of the stomach, Mr. Gamgee says: 'Warranting, as such cases do, the placing of cancer of the stomach amongst diseases curable by the knife, do they not also justify the vivisection of dogs by Shipton and Travers, who, by their experi-

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\* *Uselessness of Vivisection upon Animals*, p. 143.

ments laid the first scientific foundation of intra-abdominal surgery? Such a statement as this must be so completely qualified as to be regarded as altogether inaccurate. No form of cancer is yet known ever to have been cured, either by operation or anything else. If removed it invariably returns, and in all these cases of cancer of the stomach quoted by Mr. Gamgee, save one, the disease speedily returned and killed the patients. The one exception has not yet been under trial long enough to enable us to give an opinion. Doubtless it will have the same end as the others."

Q. *Have experiments been performed on the movements of the stomach?*

A. Yes, many such. The abdomen has been laid open, and the stomach exposed in a bath of salt solution. Cats and dogs were curarised or anæsthetised with chloroform and the stomach was stimulated.\* In the report of the Scientific Grants Committee of the British Medical Association for 1888, we read† that:—

"Dr. Angel Money has made experiments on anæsthetised curarised animals . . . the voluntary muscles were paralysed by means of curare. . . . The movements of the stomach and intestine were watched through a small window in the peritoneum. Artificial respiration was employed in the usual way. . . . The animals used were rabbits, dogs, and cats. . . . The cortex of the brain was removed by slicing and electrical irritation practised on the divided white matter."

Q. *Did not M. Schiff experiment largely on animals in this connexion?*

A. He did, and has left the records of them in his book entitled *Leçons sur la physiologie de la digestion*. In this work he says:—

"I am obliged to cause many of our dogs newly arrived at the laboratory to suffer this last operation (the division of the inferior laryngeal nerves) to prevent them giving too noisy nocturnal concerts and thus bringing discredit on physiological studies amongst the inhabitants of that quarter."‡

A visitor paid an unexpected call to Professor Schiff's laboratory at Florence, and discovered the presence of a considerable number of living dogs, with open wounds in their throats, which the Professor confessed had been made by him to prevent the animals from howling and disturbing the neighbour-

\* *Pflüger's Archives.*, Vol. XLIX., 1891, p. 162.

† *British Medical Journal*, August 4th, 1888, p. 264.

‡ *Tom.* 1er, p. 291.

hood.\* Yet notwithstanding this, M. Schiff was disingenuous enough to say before the Société Protectrice de Genève :

"I can assert with a good conscience that for more than twenty-five years no one has heard in my laboratory a single cry of pain from animals under operation."†

Q. *Experiments have been performed I believe to test the power of the stomach to digest living things ?*

A. Yes. Claude Bernard introduced the leg of a living frog through an opening made into the stomach of a dog. Dr. Pavy did the same with the ear of a living rabbit, and in both cases the objects so introduced were digested.‡

Q. *I am convinced that the surgery of the stomach as well as that of other abdominal organs has not been advanced by vivisection but I should like more information concerning the study of digestion and the gastric juice. Mr. Stephen Paget, for example, seems to attribute much of our knowledge of the process of digestion to experiments on animals.§*

A. Mr. Paget, in a long quotation from Claude Bernard,|| endeavours to show that from the end of the seventeenth century to recent times the study of digestion has been carried out by such experiments till at last we have come to understand it, but Mr. Paget, I venture to think, proves our case instead of his own. It was at first thought that digestion was a sort of "coction" or stewing process, then with Galen it was held to be a "fermentation" as of wine in a vat. Afterwards, Italian observers concluded that digestion was the purely mechanical act, a sort of grinding, like that which had been observed in the gizzard of birds. Then Réaumur, in 1752, made birds swallow food in open-work tubes filled with grain. His experiments were not very successful but he made some important discoveries and the experiments were not very cruel. By this time it was found that digestion was not a mere grinding process but was a dissolution of food by the fluid in the stomach—the gastric juice. At last it was discovered, as Bernard says, that digestion is not simply dissolution but a true chemical transformation.

\* *Ayez Pitié*, par J. C. Scholl, p. 142, foot-note.

† *Ami des Animaux* [de Genève], février, 1877, p. 3.

‡ *Text-Book of Physiology*, Landois and Stirling, 3rd ed., p. 251.

§ *Experiments on Animals*, Stephen Paget, pp. 20-31.

|| *Physiologie Opératoire*, 1879.

*Q. Was this discovery important to practical medical medicine?*

A. It was, but not till 1825, when Dr. William Beaumont, an American surgeon, became acquainted with a young Canadian who was shot in the abdomen in 1822, and recovered with a permanent opening in the stomach, which could not be closed. Dr. Beaumont took the young man, by name St. Martin, into his service and with his consent performed a vast number of experiments upon him the results of which he published in 1833. The opening in the young man's stomach was a gastric fistula, and the experiments caused him no pain because such an orifice is no more than an inconvenience, and the patient, if we may call him so, remained in perfect health for many years, whilst Dr. Beaumont introduced all sorts of food, thermometers and other apparatus into the aperture in the stomach, removing them from time to time to observe the process of digestion, temperature, &c. It was by this research and not by vivisection that our knowledge of digestion was so greatly improved.

*Q. I notice that books on physiology say that the discovery of the process of the changing of the food into chyme by the gastric fluid, and the time occupied in the process, is partly due to experiments made on living and on recently-killed animals.\* What do you say to that?*

A. The experiments on living animals taught us very little, as I have told you. Those on recently-killed animals were not vivisections and so painless, and do not concern us here. Those of Dr. Beaumont on the Canadian are the only ones which have influenced medical practice to any important extent.

*Q. Has the well-known fact of the powerful digestion of dogs influenced medical treatment of dyspeptics?*

A. The *Lancet* (February 17th, 1900) described a new treatment of dyspepsia by administering the gastric juice of dogs. But as the *Lancet* said this would entail the keeping of packs of dogs with gastric fistulæ to provide the necessary gastric juice. One patient under this disgusting treatment swallowed every day half a litre of the juice of a dog's stomach. We are happy to say the method has not become popular.

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\* *Human Physiology*, W. S. Furneaux, p. 104.

## CHAPTER XXVII.

### GLYCOGEN AND DIABETES.

Q. *What is meant by the expression, " the glycogenic function of the liver " ; it is connected I believe with the theory of diabetes, and its discovery is often said to be of the greatest importance to the treatment of that disease ?*

A. Mr. Erichsen, as spokesman of the Royal Commissioners, who dealt with the question of vivisection, asked Professor Turner the following question :—

In diabetes " it was supposed, not many years ago, that the sugar was formed in the kidneys ; it is now known by physiological experiment that the sugar may be produced by a lesion of the nervous system. Claude Bernard has shown that, if a certain portion of the brain is injured, you get sugar in the urine ; that the sugar has nothing more to do with the kidney, and is no more a kidney disease, in point of fact, than the purulent expectoration in a consumptive patient has to do with the mouth ; that the kidney merely evolves it from the system, just as the mouth ejects the purulent matter from the lungs ? " Professor Turner replied, " That is the case." (Q. 3,126.)

Q. *At what date did Claude Bernard publish his discoveries on the glycogenic function of the liver ?*

A. The results of his first experiments were published in December, 1843.

Q. *Am I to understand that before that time it was the opinion of experts that the kidneys were alone responsible for diabetes ?*

A. Mr. Erichsen was not justified in his assertion, nor is Mr. Stephen Paget justified in his assertion that before Claude Bernard (1813-1878) the pathology of diabetes was almost worthless.\* In *Hooper's Medical Dictionary* (7th Ed., 1839), four hypotheses respecting the cause of diabetes are given.

1. A morbid action of the stomach and chyle-producing viscera.

2. A morbid state of the blood, produced by a diseased action of the assimilating power.

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\* *Experiments on Animals*, by Stephen Paget, p. 27.



3. A retrograde motion of the lacteals.
4. A morbid condition of the kidneys.

So you see that the kidney theory of diabetes was only one of four that were in vogue in 1839.

But more than a hundred years ago Dr. Cullen, Professor of the Practice of Physics in the University of Edinburgh, stated that "the disease depends especially upon a defect in the assimilatory powers of sanguification," or the production of blood from the chyle.\*

Dr. Mead, the distinguished physician who lived 1673-1754, who was physician to George II., ascribed the diabetic water to "a morbid state of the liver and bile."†

Coming down to the present time we find no less an authority than Dr. Osler‡ giving amongst the supposed causes of diabetes disturbances of the liver function and defective assimilation of the glucose in the system, so that the most modern clinical research confirms those opinions of Mead and Cullen published more than a hundred years before Claude Bernard's alleged experiments on animals led to his discovery of the glycogenic functions of the liver.

Q. *Why do you say "alleged experiments"?*

A. Because in his researches he examined the livers of seven recently-dead human subjects. In three livers he determined the absolute weight of sugar, finding an average of 22·03 grammes; while in the liver of a diabetic subject where death was sudden from pulmonary apoplexy, the amount of sugar was 57·50, or more than double. The glycogenic function of the liver was thus demonstrated in a legitimate way; and in the Abattoirs he could have performed any number of *post-mortem* experiments, if confirmation or further elucidation were desired."§

Q. *What part then did Vivisection play in this research?*

A. Bernard found that by injuring the brain centre for the hepatic vaso-motor nerves—of the floor of the lower part of the fourth ventricle of the brain—in animals, he could induce glycosuria, or the formation of large quantities of grape sugar by the liver which passed into the blood and from the blood into the urine. Brunton, Pavy, Ferrier, Schiff, and other experimenters injured other parts of the animal body and

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\* Thompson's "Life of Cullen," vol. II., p. 210.

† See *Vivisection*, by Dr. James Macaulay, p. 45.

‡ *Principles and Practice of Medicine*, 1895, p. 321.

§ *Vivisection*. Prize Essay by Dr. James Macaulay, pp. 45, 46.

secured a like result. There was, however, much confusion and variation in their conclusions. Their experiments were cruel and destructive, and the lessons they taught could have been learned by more legitimate means. All the experiments merely showed that the normal secretion of sugar depends on the organs of nutrition acting in a healthy manner, and any attempt to injure them produces abnormal secretion of sugar.

*Q. What influence has all this research had upon practical medicine?*

A. Little, if any at all. One of the most valuable text-books of medicine, that of Professor Felix von Niemeyer, says\* :—

“The pathogeny of diabetes still remains obscure. The discovery of the physiologists, that sugar appears in the urine of animals after puncture of the floor of their fourth cerebral ventricle, has not yet thrown light upon the mystery. We know that the presence of sugar in the urine, whence diabetes mellitus derives its name, does not depend upon the functional abnormality of the kidneys; that the sugar is not formed in them, and that it is excreted from the blood; but we are altogether ignorant wherein the constitutional anomaly consists, in consequence of which a diabetic patient's blood contains sugar, and a healthy person's none. As the various hypotheses offered to account for diabetes are of little practical value, we shall mention a few merely of those most generally entertained.”

*Q. What effect has vivisectional research had on the treatment of diabetes?*

A. In the year 1779 Dr. Francis Hutchinson wrote to Dr. Cullen describing a case of diabetes under his care which yielded to treatment by opium amongst other things. Now opium to this day is the doctor's sheet anchor in one form or another for this malady. *Hooper's Medical Dictionary* (1839) recommended “confining the patient to an animal diet and enforcing an entire absence from every species of vegetable matter.” At the present moment this is in the main the approved diet for diabetics. It has to a certain extent been modified by recent chemical research in food-stuffs, but animal food is to-day the main staff of a diabetic's diet. Notwithstanding all the important discoveries in physiology, interesting as they are in a scientific aspect, diabetes cannot be cured by them.

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\* *A Text-book of Practical Medicine*, vol. ii., pp. 770-71.

*Q. What effect has vivisectional experiment in diabetes had upon the death-rate?*

A. The Registrar-General shall reply.

DEATH-RATE FROM DIABETES PER MILLION LIVING.

5 years 1861-65	5 years 1866-70	5 years 1871-75	5 years 1876-80	5 years 1881-85	5 years 1886-90	5 years 1891-95
29·2	31·8	35·8	40·4	51·4	62·4	69·4

## CHAPTER XXVIII.

### MYXŒDEMA AND THE THYROID CURE.

Q. *What is Myxœdema ?*

A. The dictionaries tell us that it is a diseased condition occurring in adults, generally females, characterised by a thickening of the subcutaneous tissue, most noticeable in the face, with a simultaneous dulling of all the faculties and slowing of the movements of the body. A precisely similar condition occurs in many cases where the thyroid gland has been removed for disease.

Q. *What is the thyroid gland ?*

A. It is a glandular body situated at the upper part of the windpipe or that part of the larynx popularly called "Adam's apple."

Q. *What are its uses ?*

A. So recently as 1888 very little was known as to its functions. In a well-known physiological text-book we find the following : "The functions of the thyroid gland are very obscure. Perhaps it may be an apparatus for regulating the blood supply to the head (?)."\*

There is a disease called goitre, and in many places it is common to find swelling of the thyroid constituting this disease which is sometimes associated with idiocy and cretinism. When the diseased thyroid gland is removed by a surgical operation a remarkable depraved condition of nutrition of the body called "cachexia" often supervenes. It had been discovered in the hospital at Berne, in Switzerland (where goitre is prevalent), that in eighteen cases of complete removal of the enlarged thyroid gland a condition which we now know as myxœdema followed.

Q. *This I presume is what is referred to in the explanation you gave me from the dictionary ?*

A. Yes ; and it is important to keep this fact in mind, because it is commonly asserted that all we know about

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\* Landois and Stirling, *Human Physiology*, 3rd ed., 1888, p. 155.

myxœdema and its cure or relief has been due to experiments on living animals. Remember that the relationship between the diseased thyroid and the condition we now term myxœdema was a discovery of surgery in legitimate hospital practice for the relief of human suffering, and had nothing to do with animal experimentation.

*Q. Were no attempts made to discover the chemical or other properties of the gland before this time?*

A. Mr. King made certain researches in 1836, and showed that the thyroid secreted a peculiar fluid, which found entrance to the general system through the lymphatics. Afterwards Crede, Reras, Alberoni, and others made further researches in the same direction.

*Q. What was the next step?*

A. Mr. Stephen Paget tells us\* that Mr. Victor Horsley commenced some experiments in 1884, and "was able, by removal of the gland to produce in monkeys a chronic myxœdema, a cretinoid state, the facsimile of the disease in man." But the surgeons at the Berne Hospital had settled all this. Mr. Horsley (now Sir Victor Horsley) took out the thyroid from a donkey and the animal died in two hundred and five days, in a miserable condition from emaciation and weakness; the creature was unable to stand, tremors and twitching supervened, muscular rigidity was observed, the paralysis increased, and the animal succumbed.† These experiments therefore only proved what was already known that the thyroid gland exercises some important function in the animal economy.

*Q. What did the vivisectors do towards discovering a cure for myxœdema?*

A. It is said, I do not know with what precise amount of truth, that in a London laboratory certain dogs from whom the thyroid had been removed and who were in consequence wasting away, discovered and ate a quantity of thyroids first removed from another set of animals, and that they, in consequence, began immediately to recover. Experiments were made by transplanting thyroids in different parts of the animals' bodies, and in many cases the most astonishing improvement in their health took place.

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\* *Experiments on Animals*, p. 193.

† *Brown Institution Lectures*, by Prof. Horsley, *Lancet*, December 18th, 1886.



*Q. But they could hardly treat human patients by such an operation?*

A. No, and it seemed that no cure would be found for myxœdema, until at last Dr. Murray, of Newcastle, by a happy inspiration, found that an injection of the extract of the thyroid gland of a sheep into myxœdemic patients would avert death and improve their health, but the treatment must be persisted in regularly or the patient relapsed. This is not quite the same as a "cure" although a most valuable remedy.

*Q. How are the sheep's thyroids procured?*

A. From the butcher's after the animals are killed.

*Q. What was the next step?*

A. The extract of the glands is put up in tabloids, and is taken by the mouth, when it is found to be equally as efficacious as the injections.

*Q. The rôle of the vivisector in all this seems singularly small?*

A. Microscopically so as regards the results, though the research in one form or another has involved great suffering to countless animals.

*Q. Has it occurred to physiological chemists to try to discover the particular principle in the thyroid which is so important to the animal's health?*

A. Professor Baumann, of Fribourg, has succeeded in isolating the active principle of the thyroid gland of the sheep, which turns out to be simply iodine in an organic specific combination. Now it is a remarkable fact that so long ago as 1863 iodine was the best treatment in vogue for thyroid disease and cretinism. Had chemical researches been originally made with a view to the discovery of the active principle of the gland, its purpose might have been discovered long ago and all the experiments on animals dispensed with. But vivisection is considered to be the royal and the short cut to medical advance, and so hinders rather than aids the progress of the healing art.

## CHAPTER XXIX.

### ANEURISM AND HUNTER'S DISCOVERY.

Q. *What is meant by an Aneurism?*

A. Briefly an aneurism is the dilatation of an artery. It is a soft tumour arising from the expansion of an artery acting on a part weakened by disease or injury. A tumour is formed which contains blood, and the walls of this tumour are composed either of the tissues of the vessel or those which form its sheath and immediately surround it.

Q. *Is this a dangerous condition?*

A. Indeed it is. The tumour is liable to give way suddenly, and the patient may die within a few minutes or hours.

Q. *Is it true that the great surgeon John Hunter discovered the method for the cure of aneurism by experiments on animals?*

A. Vivisection had absolutely nothing whatever to do with Hunter's discovery. It is true that after making it he experimented on animals. Sir William Fergusson, F.R.S., Sergeant Surgeon to Queen Victoria, told the Royal Commission\* that "Hunter's first experiment, if it might be so-called, was done on the human subject, and it was long after he had repeated his operation on the human subject, and others had repeated it, that the fashion of tying arteries on the lower animals originated or was developed."

Mr. Lawson Tait, F.R.C.S., wrote: "As the arteries of animals never suffer from the disease in question, experiments upon them could not have helped Hunter in any way whatever. Sir James Paget, who has lately appeared as an ardent advocate for vivisection, and, therefore, may be appealed to by me as a witness not biased to my view, has recorded his opinion in the Hunterian Oration given at the College of Surgeons in 1877, that Hunter's improvement in the treatment of aneurism 'was not the result of any laborious physiological induction; it was mainly derived from facts very cautiously observed in the

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\* *Evid. Roy. Com.* (London, 1876), Q. 1,024-5.

wards and deadhouse.' In this opinion Sir James Page undoubtedly correct."\*

"Hunter tried his best to induce aneurism on the low animals and failed."†

*Q. Was not aneurism treated surgically before the time of Hunter?*

A. It was. A great surgeon named Antyllus, who lived about A.D. 300, treated aneurism by tying the artery above and below the swelling or sac, and turning out the contents. Performed as it was without anæsthetics or antiseptics it was an operation attended with great mortality. Then a famous French surgeon Anel, who lived 1679-1730, improved the operation by tying the artery just above the sac on the side nearest the heart, this proved very dangerous from various causes. Hunter in 1785 tied the artery just as Anel did but much further away from the aneurism, his object being not to cut off the blood-supply entirely, but to allow it to enter it with a greatly diminished force, and so to deposit a clot that gradually becoming consolidated and so the sac is transformed into a mass of firm fibroid tissue.‡

*Q. It seems to me that the difference in these operations is mainly the question of the distance of the ligature from the seat of the disease?*

A. Yes. Hunter chose a healthier portion of the artery than that nearest the sac.

*Q. What had to be done when there was an aneurism in the leg which could not be cured?*

A. There was nothing to do but amputate the limb. Rather than do this Hunter in his first case of the kind determined to make the experiment of tying the femoral artery. The patient recovered. Sir Astley Cooper, in his "Lectures on Surgery," says that it was this successful case which induced Hunter to perform his experiments on the low animals.

*Q. If you tie a main artery you must cut off the blood-supply to a limb. How, then, is the circulation maintained?*

A. When an artery is obstructed the vessels in its neighbourhood become enlarged, and the parts beyond the ligature

\* *The Uselessness of Vivisection.* New Ed. (London, 1883), p. 1.

† *The same book*, p. 34.

‡ See *A Manual of Surgery.* Rose & Carless, 2nd Ed., 1881, pp. 267-68.

become nourished by what anatomists call the collateral circulation.

*Q. How did this fact become known to Hunter?*

A. He experimented on one of the deer in Richmond Park, and the result led him to the discovery of the collateral circulation. That much must be admitted. After the vivisections which he performed, he says, "I have frequently tied the femoral artery in animals without injury; why should not I put a ligature on the artery in the same way in the human subject?" On this Dr. Wall says\* :—

"It is palpable that, as he had already done this with complete success in the human subject the question was needless, and the experiments were as unnecessary as cruel. If his object was to see what effect the ligature had on the femoral artery, or by what means the collateral circulation was carried on, he could have ascertained this upon the human body, for the patient upon whom he first operated by the new method died about a year after. The body was examined, and it was found that the femoral artery was completely obliterated up to its division into the profunda femoris, and the method by which the blood was carried to the parts below the ligature could be made out by careful dissection."

*Q. Do you not think that it is probable that Hunter experimented upon animals with the object of convincing his opponents by ocular demonstration of the value of his discoveries?*

A. No doubt the neglect of morbid anatomy amongst the earlier physiologists was responsible for a great deal of vivisection.

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\* *Painful Experiments, &c.* Prize Essay, p. 279.

## CHAPTER XXX.

## THE LIGATURE OF ARTERIES.

*Q. I have often been assured that experiments on animals have led to great improvements in the methods of tying the arteries for the arrest of bleeding. What do you say to that ?*

A. Savages arrest severe hæmorrhage by applying heated stones to the bleeding vessels. The application of heat for this purpose was general in Europe up to the time of Ambroise Paré, "the father of French Surgery" as he was called. He lived 1509—1590 and is considered as the first who regularly employed the ligature to the blood vessels after amputation. He declares in his *Apologie* that the invention was due to the ancients, and he explains their use of it, although he ascribes to inspiration of the Deity his own first adoption of the practice.\*

*Q. Did the ancients tie bleeding vessels ?*

A. Galen, the profound anatomist and physiologist, who was born A.D. 170, mentions the practice as safe and that even the carotid arteries may be tied with impunity. Then Antyllus, who lived about 300 A.D., and who has been called one of the greatest of the world's surgeons, ligatured the arteries for aneurism and tied the vessels for operations on the neck before dividing them.

*Q. Have any surgical instruments been discovered at Pompeii or Herculaneum which tend to prove that the ancients knew how to deal in this way with bleeding arteries ?*

A. Mr. Cockayne says in his *Preface to Saxon Leechdoms* (Vol. I., p. xxiii.) concerning such an instrument : " It has been specially considered by Prof. Benedetto Vulpes (1847) who thinks it may also have been intended to take up an artery. The Greeks, he observes, as appears by an inscription dug up near Athens, were able to tie an artery in order to stop hæmorrhage, and words implying so much are found in a treatise of Archigenes (A.D. 100), existing in MS. in the

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\* *Origin and Growth of the Healing Art.* By E. Berdoe, p. 368.

Laurentian library at Florence, '*the vessels carrying (blood) towards the incision must be tied or sewed up.*' Near the end of the sixteenth century a French surgeon was the first to restore the ligature of the artery, and the instrument he used was very similar to the forceps in the museum at Naples."

*Q. This seems conclusive, but is there not a modern invention for arresting bleeding which is superior to tying and consists in twisting the artery?*

A. Certainly there is such a practice, and is the routine in surgical operations involving the severing of arteries; it is called "torsion." It is, however, not a modern invention at all, both torsion and ligature of arteries were known to the ancients, but they fell into abeyance, and were rediscovered. Rufus, of Ephesus, the anatomist who lived A.D. 98—117 practised both methods. Ætius, a Greek medical writer of the sixth century, treated bleeding arteries by twisting, as we do now, and by tying.

*Q. I see that Ambroise Paré was quite right in saying that the invention of arresting hæmorrhage from the blood vessels was due to the ancients. Have we any evidence that experiments on animals have taught us other modes of arresting bleeding?*

A. None were needed, though acute observation of the ways of animals no doubt taught savage man many lessons in surgery. As I have elsewhere shown the snipe has often been observed engaged in repairing damages to its limbs by surgical treatment.\* M. Fatio, in an address delivered to the Physical Society of Geneva, quoted the case of this bird who with beak and feathers makes a very creditable dressing, applying plasters to bleeding wounds, and even securing a broken limb by means of a stout ligature. On one occasion he killed a snipe which had on the chest a large dressing composed of down taken from other parts of the body, and securely fixed to the wound by the coagulated blood. Twice he brought home snipe with interwoven feathers strapped on to the site of fracture of one or other limb. M. Magnin records the case of a snipe which was observed to fly away with a broken leg, and was subsequently found to have forced the fragments into a parallel position, the upper fragment reaching to the knee and secured by means of a strong band of feathers and moss intermingled. The observers were

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\* *The Origin and Growth of the Healing Art.* By Edward Berdoe, p. 4.



particularly struck by the application of a ligature of a kind of flat-leaved grass wound round the limb in a spiral form, and fixed by means of a sort of glue.

Livingstone says that the chimpanzee, soko, or other anthropoid apes will staunch bleeding wounds by means of their fingers, or of leaves, turf, or grass stuffed into them.

## CHAPTER XXXI.

## EXPERIMENTS WITH DRUGS.

*Q. Sir Lauder Brunton, M.D., in his work entitled "A TEXT-BOOK OF PHARMACOLOGY, THERAPEUTICS, AND MATERIA MEDICA," says (p. 39) "Almost all our exact knowledge of the action of drugs on the various organs of the body, as well as the physiological functions of these organisms themselves, has been obtained by experiments on animals." What do you say to that?*

A. Medicine is not an exact science, and the most accurate acquaintance with the action of a drug upon a particular organ of the body very often indeed fails to assist the physician in curing the malady from which that organ may be suffering. Again, many of our most valuable remedies have been used in medicine for long ages before we came to know their precise physiological action, and if the doctor had to pause to-day before prescribing for his patients, to settle in his mind what is the precise physiological action of his remedies, his patient would often die or recover before he had finished the solution of the problem. Quinine, for example, was used for the cure of ague and malarious fevers some two hundred years before the bacteriologists discovered how it acted in such cases. If the Jesuits who discovered the properties of cinchona bark in the forests of Peru, had neglected to use it as a medicine until scientists had found out what it did when taken into the system, they would have deprived the world of the benefits of Peruvian bark from the year 1628 till 1880, when Laveran discovered the small organism—a protozoon—to which malaria is due.

*Q. Is not malaria caused by mosquitoes?*

A. That is the theory. The spores of the disease are conveyed from man to man by the mosquito.

*Q. Have experiments on animals been made in this connection?*

A. Surgeon-Major Ronald Ross experimented in India, using grey mosquitoes and proteosoma-infected birds. Mos-

quitoes fed on the sparrow with numerous proteosoma were found to have become the intermediate hosts for the malaria germ. When these mosquitoes fed on healthy sparrows they infected them. When mosquitoes were allowed to feed on malarious patients and afterwards on healthy men who submitted voluntarily to the experiment, these persons contracted malaria.

*Q. Then the prevention of malaria will depend upon the prevention of mosquito bites?*

A. Yes, but quinine is still the specific for malaria, and the discovery of its germicide action, however interesting to physicians, has in no way altered the treatment as far as the patients are concerned. Mr. Stephen Paget says, "Before leaving the subject of malaria, it must be added that the discovery and study of the parasite which causes it has cleared up the mystery of the specific action of quinine upon the disease. It operates simply by its germicidal effect upon the microbe. But beyond this we have now a clue which we never had before to guide us to the most advantageous manner of administering the drug."\*

*Q. Can you illustrate the point by other medicines than quinine?*

A. Long before the disease known as myxœdema was properly understood and treated by thyroid extract, iodine was successfully used in cases of the malady. Then Surgeon-Major Holmes reported in the *Lancet* for October 10th, 1863, its successful treatment with red iodide of mercury, and Dr. Coindet of Geneva had before this used the iodide treatment with great advantage. It did the patients good, but the doctors had no exact knowledge how it operated. When modern research had disclosed the true pathology of the disease and the thyroid gland had been submitted to exhaustive analysis, it was found that the healthy organ contained as its active principle a peculiar form of iodine called thyroïdin. We now know how it was that iodine relieved patients suffering from atrophied thyroid glands.

*Q. When the action of a drug has been ascertained by a physiological experiment, is the physician who uses it solely guided by that knowledge?*

A. That would be very unsafe, and even dangerous in many cases. The action of a potent poison on an animal and

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\* *Experiments on Animals.*—Stephen Paget, p. 185.

on a man is often widely different. The physiological and pharmacological sciences are valuable in their proper places, but the physician who depended upon them alone would make many mistakes. Professor Huxley in a letter to the *Times* (Nov. 18th, 1889), said on this subject, "Mr. Spencer\* assumes that in the present state of physiological and medical science, the practitioner would be well advised who should treat his patients by deduction from physiological principles ('absolute physiological therapeutics,' let us say), rather than by careful induction from the observed phenomena of disease and of the effects of medicines. Well, all I can reply is, Heaven forbid that I should ever fall into that practitioner's hands; and if I thought any writings of mine could afford the smallest pretext for the amount of manslaughter of which that man would be guilty, I should be grieved indeed."

Q. *When a drug has been ascertained by experiment to exert some definite physiological action on a particular part of the body, does that constitute its therapeutic action?*

A. Not always; for example, the well known drug "antipyrine" was originally introduced, as its name implies, to reduce the temperature in febrile diseases. But physicians soon discovered in actual practice that it had a much more valuable property as a reliever of pain. It is largely used now as a valuable nervine sedative, but it was not experimentalism on animals that led to this use of the remedy. Again, belladonna is physiologically incompatible with opium, it is actually an antidote in cases of poisoning by that drug, in the same manner opium is an antidote for belladonna. Notwithstanding these facts, belladonna and opium, and their alkaloids, atropine and morphia, are frequently employed in conjunction with much success. Sir Lauder Brunton† says of belladonna and cantharides in the treatment of enuresis, "Now those two drugs possess an entirely opposite action; belladonna tends to act as a sedative to the bladder, lessening its irritability; cantharides acts as a stimulant to the bladder, greatly increasing its excitability. Here again it would seem as if it were perfect nonsense to attempt to treat a condition by two entirely opposite remedies, and yet practice shows that they both are successful in many cases."

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\* In reply to Mr. Herbert Spencer's letter in the *Times* (November 15th, 1889).

† *The Action of Medicines* (p. 548).

Q. *What is a specific?*

A. A remedy which has a special power in a particular disease; an infallible remedy.

Q. *And what is empiricism?*

A. The system which, rejecting all *à priori* knowledge rests solely on experience and induction, it is the dependence of a doctor on his *experience* alone without a regular medical education. The apologists of vivisection have a shorter explanation for it; they call it quackery.

Q. *Who gave us our specifics?*

A. Certainly not the vivisectors. I know of no new drug which has been introduced into medicine in consequence of laboratory experimentation which can be called a specific. On the other hand, we have many specifics which have come into medical use by happy accident.

Q. *Can you give me a list of these?*

A. I cannot do better than quote to you the words of Professor Bouchard from a paper which he read at the meeting of the Medical Congress held at Cairo in December, 1902. He says, "Empiricism has given us opium, which does not often cure, but which relieves almost always, and the empiricism of olden days has given us nearly all our drugs, among which are several which cure, such as quinine, mercury, the iodides, arsenic, colchicum, and salicin—all drugs the use of which we have learnt by happy accident. Each of these drugs cures a special disease, and almost exclusively that disease, its action being specific. Our forefathers did not know or even suspect the reason, but most of us to-day know the secret of their action. They influence the poison by means of constitutional treatment."<sup>\*</sup>

Q. *And what have the experimenters contributed?*

A. They have told us that in consequence of their prolonged and elaborate researches we may properly use these remedies for the diseases which have been treated by them successfully long years before the pharmacologists came upon the scene.

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<sup>\*</sup> *The Lancet*, February 7th, 1903.

*Q. But I presume they have investigated the physiological action of these drugs by experiments on animals and ascertained precisely what parts of our economy they affect?*

A. Certainly. Mr. Stephen Paget says,\* "Aconite, belladonna, calcium chloride, colchicum, cocain, chloral, ergot, morphia, salicylic acid, strophanthus, the chief diuretics, the chief diaphoretics—all these drugs, and a host more, have been studied and learned (*sic*) by experiments on animals."

*Q. And how have the experimenters agreed about their researches?*

A. They have disagreed profoundly. For example, Prof. Wood says about aconite that "the experiments by Mackenzie upon frogs have yielded apparently contrary results to those of Böehm and Wartmann as to the effect of aconite upon these animals."† Dr. Ringer says that "very diverse statements are made concerning its action on the nervous system."‡ See for much further proof of experimenters' contradictory results Berdoe's "Futility of Experiments with Drugs on Animals," p. 7.

*Q. And what about belladonna?*

A. Wood says§ that none of the experiments seem decisive, and that their results are not in accord with clinical experience. Dr. Erlenmeyer is opposed by Dr. Brown-Sequard and Dr. Harley, and Meuriot and Harley contradict each other upon the results of their experiments.

*Q. And of other drugs probably similar conflicting evidence exists?*

A. Yes, and to a much greater extent than I can waste time in telling. Of calabar bean the most conflicting testimony is given by physiologists who have experimented with it. Experimenters with chloral hydrate contradict each other about its physiological action in the most bewildering manner.|| Of cocain, say Stillé and Maisch,¶ "So far as experiments have been undertaken, they do not agree in their

\* *Experiments on Animals*, p. 201.

† Wood, *Therapeutics*, p. 177.

‡ *Handbook of Therapeutics*, p. 397, 5th ed.

§ *Therapeutics*, p. 252.

|| See *American Journal of Insanity*, July, 1871, and *American Journal of Medical Science*, April, 1870.

¶ *National Dispensatory*, 2nd ed., p. 554.



physiological results." Concerning digitalis the experimenters Boehm and Ackerman contradict each other, and Brunton contradicts Schmiedeberg and Boehm. Ringer says, "According to Saunders, Jörg, Hutchinson, and others, digitalis in moderate doses in the first instance, quickens the pulse, though other observers deny this effect."\* Of hemlock Ringer says, "Concerning the action of this poison on the heart, very conflicting statements have been made."† About opium and its various alkaloids many physiologists have arrived at conclusions which were promptly contradicted by another set of equally competent and painstaking observers. Irreconcilable conclusions were arrived at by Claude Bernard and Baxt concerning their experiments with papaverine.‡ Rutherford's experiments, undertaken to reconcile the conflicting results of other experimenters, "led to diametrically opposite results."§ And so we might go on indefinitely, yet Mr. Paget has the hardihood to say that "all these drugs, and a host more, have been studied and learned (*sic*) by experiments on animals."

*Q. Are experiments with drugs necessarily cruel?*

A. In many cases they involve much cruelty. For example, in Professor Rutherford's notorious experiments on the biliary secretion of the dog, they were in his first series all made on dogs who had previously fasted eighteen hours. The animal was first paralysed by injecting a solution of curare into the jugular vein; the windpipe was then opened, a tube fixed within it to connect it with a machine for maintaining artificial respiration: then the abdomen was opened, the stomach and duodenum were moved aside, and the edge of the liver was raised, the common bile duct was dissected out, divided, and a glass tube inserted into it; the gall bladder must now be compressed so as to fill the tube with bile; and in order to prevent the return of the bile to the gall bladder, the cystic duct was clamped. When all this was arranged, the wound in the abdomen was closed, and the animal wrapped up in cotton wool, so as to restore it, if possible, to its normal temperature. This operation occupied about half an hour to perform; no chloroform or anæsthetic was given, the dog being merely curarised, and consequently entirely sensitive to the pain it

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\* Ringer, *Therapeutics*, p. 411, 5th ed.

† *Op. cit.*, p. 442.

‡ Wood, p. 223.

§ Stillé and Maisch, p. 1124.

underwent.\* The wound in the abdomen was opened several times during the course of the experiment to inject the curare.

Q. *Why was no anæsthetic given?*

A. For the reason I explained in a previous conversation on curare. Professor Rutherford, in his examination before the Royal Commission, said (Q. 2,932) he "did not dare give chloroform because of the risk of having the results possibly interfered with." Asked if anæsthetics would have retarded his investigation, he replied (Q. 2,913), "I could not have given them. It has been proved by Röhrig that the action of such a narcotic as opium diminishes the action of the liver; therefore if I had given them, and given a substance like colchicum, I might have got results upon which no definite conclusion could have been founded. It is sometimes impossible to arrive at correct therapeutic knowledge if two substances be given."

Q. *Would Dr. Rutherford's experiments entail great pain?*

A. Any one who has seen a patient suffering from an attack of gall-stone colic would be able to estimate the agony of the victim of this research.

Q. *Were the results of these experiments very helpful to practical medicine?*

A. I have in a former work told the history of their most misleading results.† I will read you the story:—

"The experience of generations strongly supports," says Dr. Ringer (*Materia Medica*, 12th Ed., p. 243), "the general conviction that in some diseases calomel, as well as other preparations of mercury, does increase the bile." But experience and clinical observation count for little with the experimental physiologists. Drs. Hughes-Bennett and Rutherford performed a very large number of cruel and excessively painful experiments on the livers of dogs. The abdomen was cut open, and a glass tube tied into the bile duct, with barbarous attendant circumstances, which placed the animals in an abnormal condition; mercurials and other drugs were inserted in the cut intestines to show their effects. The operators came to the conclusion that the doctors had been all wrong in their conclusions about calomel, and they proved to their

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\* See *Painful Experiments on Living Animals*, by Dr. Abiathar Wall (Prize Essays), p. 233, to which I am indebted for the above account.

† *Futility of Experiments with Drugs on Animals*, by E. Berdoo, p. 27.

own satisfaction that it did not increase the secretion of the bile. Of course, no physician worthy of the name paid the slightest attention to these conclusions, but went on administering what his experience had proved to be so valuable; and fortunately so, for it ultimately dawned upon the intellects of Messrs. Bennett and Rutherford that there was all the difference between administering calomel by the stomach, thereby mixing it with the gastric juice, and cutting open the upper part of the intestines and inserting the drug there. Rutherford also found that the curare given to keep the animals quiet, diminished the bile and made the heart's action weak and irregular—so that, as Mr. Reid said in the House of Commons, April 4th, 1883:—"The result of these experiments was simply nothing at all."

*Q. Apart from experiments such as you have described as connected with the bile, and those of which you told me in relation to the kidney, is the testing of drugs on animals in the more normal manner usually of a cruel nature?*

A. Mr. G. F. Dowdeswell made experiments upon cats, dogs, guinea-pigs, and rabbits with the salts of vanadium. "In experiment No. 3, for forty-six days a dose was administered in food, which produced excessive constitutional disturbance—vomiting, purging, and emaciation; at the end of that period the animal was killed, and fatty degeneration of the organ [the liver] was found distinctively pronounced." \* Dr. Stockman experimented on animals with borneol (a form of Borneo camphor). It is on cats, we learn, that the symptoms produced "may be observed in the most typical and aggravated form." The animal was at first intoxicated, then trembling followed, and violent convulsions. "Chloroform," we read, "completely stops the convulsions." The animal at intervals has "true epileptic convulsions." "The animal may remain in this condition for about forty-eight hours." † Dr. W. B. Platt, M.D. (Harvard), M.R.C.S. (Eng.), made experiments on the action of resorcin on dogs, rabbits, and frogs. The animal trembles, pants, growls, whines and howls, falls to the floor, cannot walk, makes biting motions, struggles, yelps, and dies in an hour and forty-nine minutes. ‡ Dr. John G. McKendrick and Mr. W. Snodgrass performed experiments with carbon monoxide of nickel upon frogs and rabbits. In the case of a rabbit a severe convulsive seizure, with gasping, took place, and the animal died after a period of four hours eighteen minutes,

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\* *Journal of Physiology*, vol. i., p. 260.

† *Ibid.*, vol. ix., pp. 70-71.

‡ *American Journal of Medical Science*, Jan., 1883, p. 100.

in "a severe convulsive spasm." \* Drs. Ringer and Murell experimented with the drug called *pilurî*. Amongst the results we read: "We next injected six minims of the 1 in 20 solution under the skin of a moderate-sized cat. In one minute it staggered much in walking, and the breathing became panting. In eight minutes the movements were quite stiff, the legs seemed without joints, and the cat seemed walking with sticks, not legs. The limbs were evidently rigid from strong muscular contraction. The respirations were 130 in the minute, and the mouth was covered with foam." †

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\* *British Medical Journal*, June 6th, 1891.

† *Journal of Physiology*, vol. i., p. 377.

## CHAPTER XXXII.

## THE PRESENT CONDITION OF THE HEALING ART.

*Q. As we are concluding our conversations on Vivisection, I should like to have your candid opinion on the present condition of medicine and surgery especially in relation to experiments on animals.*

A. If by this question you mean to ask what I think of the new drugs and methods of treating disease which have been introduced in recent years, I am compelled to reply that the healing art is in a state of great confusion, and that physicians who pay attention to the host of new remedies daily being introduced with much booming from pharmacological laboratories are liable to be bewildered by the contradictory opinions as to their merits. If, however, you desire to know what are the prospects of the medicine of the future, I believe they are very bright, because we are inclining to the belief that drugging is an evil to be avoided, and the more educated physician studies how little harm he can do and how much he may safely leave to Nature and good nursing.

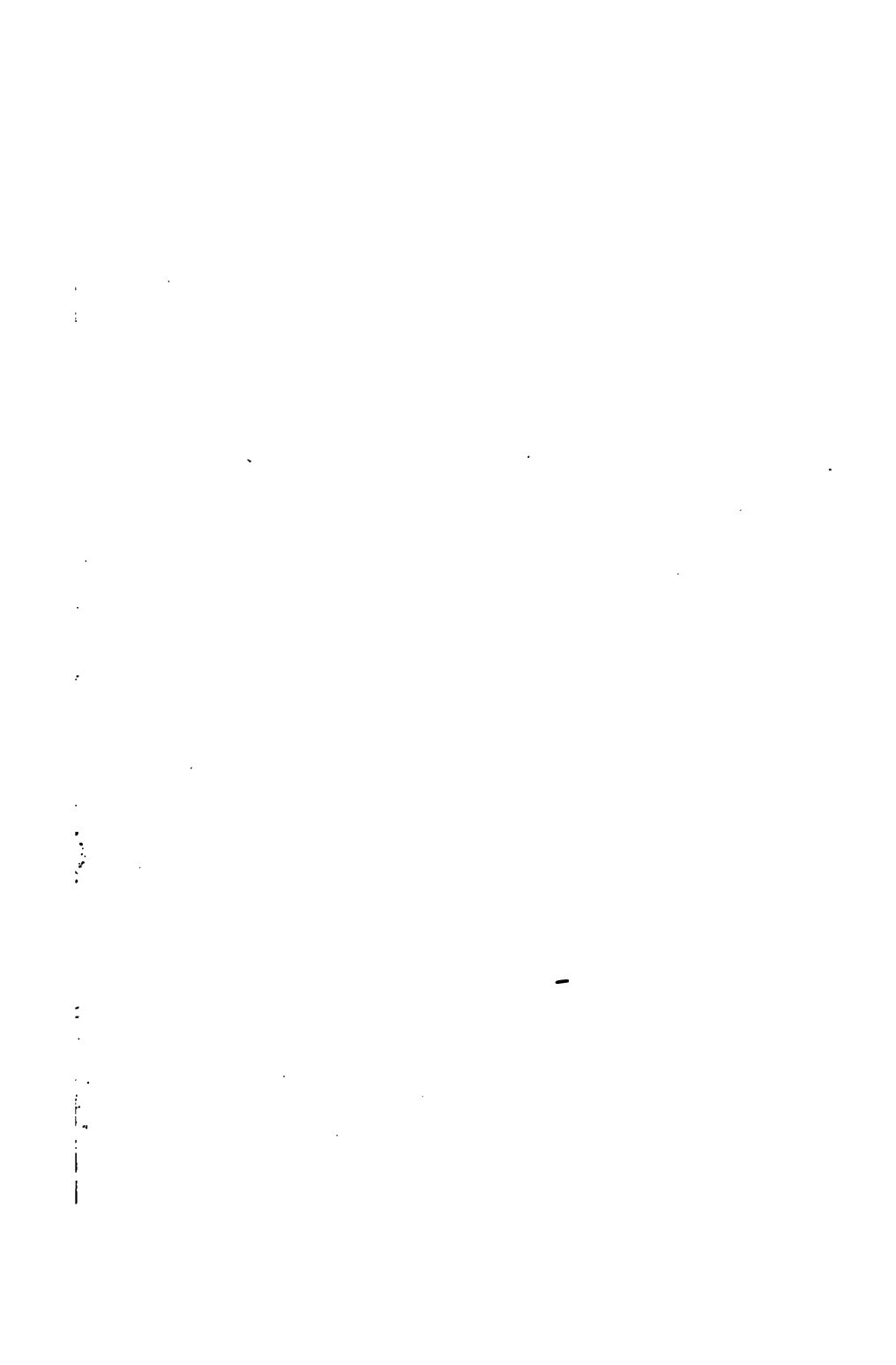
*Q. Do you mean to say that notwithstanding the multitude of new "tabloid" and similar compressed drugs which nearly everybody swallows, physicking is going out of fashion?*

A. Not with the public, but certainly with the better educated doctors. A great number of new medicines have recently been invented for obtaining relief from pain and inducing sleep. For the most part they have been introduced to the profession from foreign laboratories after experimentation on animals, they have been largely advertised, and as largely prescribed, and now that patients are becoming clever at reading prescriptions, they have taken the matter largely into their own hands, with serious and even fatal consequences. The indiscriminate use of the hypodermic syringe has wrecked thousands of intellects and ruined many constitutions, so that doctors are becoming alarmed at the abuse of drugging, and are using all their influence to arrest the evil.

*Q. Have physicians of eminence published opinions in support of this?*

A. Certainly. The "Medical Annual" for 1903 in its opening pages has an article by Hobart Amory Hare, M.D. (Professor of Therapeutics in the Jefferson Medical College of Philadelphia, Physician to the Jefferson Hospital), entitled "A Review of Therapeutic Progress for 1902." He says: "With the increasing knowledge which we possess concerning the measures employed by the system for its protection from the outset and progress of disease, and when we further regard the intricate subject of the development of toxins and anti-toxins, it is perceived that the physician who throws a drug of uncertain effect into the cogwheels of this wonderful mechanism, must be possessed of extraordinary ignorance or colossal impudence. It is seen that the old motto, '*Nil nocere*' ('Do no harm') is a very important one to be followed by the practitioner, and in every case the physician should look back upon his plan of treatment in each instance, not only to make note of the factors which were potent for good, but also to expose to his own view things that had better have been left undone." Many of the new remedies produced are neither as efficacious nor as harmless as those of our forefathers, but the activity of the manufacturing chemists who employ physiological experimenters to aid them in their business is for ever at work bringing out new medicines, which are recommended by physicians because of their alleged action on animals. The doctors, being possessed of the physiological enthusiasm, believe the advertisers, and purchase their productions. The fashions in drugs are as liable to change as those of dress; one remedy supersedes another so rapidly that such a feeling of uncertainty about medicines has arisen in many minds that the science of therapeutics is in danger of being neglected. It is probably for this reason that the system of serum treatment has met with such favour. As nobody knows how the anti-toxins act, the method of treating disease by their agency has an air of magic, and the unknown is considered the magnificent. Be this as it may, the medical arts are in a state of transition. The only chance for the science of healing lies in the obedience of the doctors to the caution of the sign-boards at American railway crossings, "Stop! Look and Listen!"







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